Summer 1960 Vol I No 3 *$2.00*

The International Quarterly of the Society for the History of Technology

Controversy:

INVENTING AND PATENTING

S. COLUM GILFILLAN (*Sociologist*)

VERSUS

JACOB SCHMOOKLER (*Economist*)

I. JORDAN KUNIK (*Patent Attorney*)

Robert S. Woodbury on "The Legend of Eli Whitney and Interchangeable Parts"

Reviews by R. J. Forbes, A. R. Hall, and others

Published by the
WAYNE STATE UNIVERSITY PRESS
for the
SOCIETY FOR THE HISTORY OF TECHNOLOGY

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Published quarterly in Winter, Spring, Summer, and Fall by Wayne State University Press, Detroit 2, Michigan

Application for second class mail privileges is pending at Detroit, Mich.

Publication of this journal has been made possible by a subscription guarantee from the Wilkie Foundation, Des Plaines, Illinois.

Editorial Office: The Society for the History of Technology, Room 311, Main Building, Case Institute of Technology, Cleveland 6, Ohio.

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TECHNOLOGY and CULTURE

volume I, no. 3 · summer 1960

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The next issue of *Technology and Culture* will be devoted to reviews of *A History of Technology*, edited by Charles Singer, E. J. Holmyard, A. R. Hall, and Trevor I. Williams. Representing the first large-scale attempt to cover the development of technology, these five volumes have already become the standard reference work in the field. This review number of *Technology and Culture* will attempt to assess the contributions of the Singer volumes—as well as to point out their shortcomings.

The reviews will be prefaced by an article by Dr. Singer, telling how and why *A History of Technology* came to be written, and by Dr. Hall's account of some of the problems encountered in organizing and editing a collaborative project of such monumental dimensions.

Then will follow general reviews by authorities such as Lewis Mumford, Lynn White, jr., Robert S. Woodbury, and Clement W. Meighan. Special fields within the history of technology will be covered by other experts: Carlo Panseri, Thomas J. Higgins, Reynold Wik, Aubrey Burstall, and many others.

Finally there will be a comparison of the Singer volumes with comparable multi-volume histories of technology which have commenced publication, or are soon to be published, in France, Itatly, and Russia.

Controversy

AN ATTEMPT TO MEASURE THE RISE OF AMERICAN INVENTING AND THE DECLINE OF PATENTING

S. C. GILFILLAN*

SINCE AMERICAN INVENTION has become a major and increasing part of the world's technical progress, it would be worthwhile if we could measure the yearly rate of American inventing. One of the many uses for such a measure of inventing would be to compare the patent system with its rival institutions for the support of invention; my study indicates that those other supports, chiefly government inventing, have come heavily to outweigh the patent system.

I

Others have previously attempted to assess the rate of inventing. Some, especially Schmookler,[1] have counted patents, although this method involves the quite unwarranted assumption that the average patent of today goes with the same amount of invention as in, say, 1880—both the same amount of invention in itself, and as a representative of the vast mass of contemporary, unpatented invention. A much sounder assumption would be that in a recent year the patents granted to residents of Connecticut bore the same ratio to that state's inventing as the ratio between patents and inventing in North Dakota in the same year. Connecticut is responsible for ten times as many patents per capita because it is a manufacturing state while North Dakota is agricultural. The difference between

* Dr. S. Colum Gilfillan, a former Research Associate in Sociology at the University of Chicago, is the author of *Sociology of Invention.* This article summarizes part of a study which is being prepared for publication by the Subcommittee on Parents, Trade-Marks, and Copyrights of the U. S. Senate Judiciary Committee. The study will be one of a series of monographs, under the editorship of Professor John C. Stedman, investigating the patent system with a view to possible reforms.

these two states is quite comparable to that between the United States of 1880 and of today. Hence one would expect American per capita patenting to have risen ten-fold; instead it has declined to 79 per cent of the 1880 per capita figure, suggesting that only one-thirteenth as large a share of American inventing is being patented today as in 1880. Later, when I advance a much better measure of invention, we shall find the indicated decline of the patent/invention ratio to have been still steeper.

Others have sought to measure the progress of invention by listing great inventions and discoveries and then counting the achievements per unit of time. Darmstaedter's list[2] of inventions since ancient times is most often employed for this purpose, but Streit's[3] is more complete for the last two centuries. I have tried this method on the Streit list and on my own unpublished list of the 500 socially important inventions since 1782. Analysis of the Streit list by this method shows no increase at all in American inventing and discovering, not even in proportion to population growth, although our country's indicated share of the world's output rose from 37 per cent to 88 per cent in 1880-1950. Indeed, the entire world's output declined sharply, according to both lists! Such a finding is evidently absurd; Streit's and my lists are obviously biased in favor of older achievements.

Amid choices so subjective, such distortion is easily explainable. In the first place it takes years for the importance of a "great" invention to be recognized if, as is usual, we date the invention at the time of its first patenting or operative working out. For instance, two inventions to be recognized as "great" in the future will be, I think, the voice-operated writing machine,[4] and clear fused quartz. The conventional dates for these inventions would probably be 1907 or 1916, in the case of the first, and 1901. But no list of great inventions yet published includes those, because their future importance is still not commonly recognized. Today, the significance of the helicopter is acknowledged, but it would be dated not in recent times, but before 1784, when a Chinese coäxial "flying top" was exhibited in Europe, or perhaps dated as of the First World War, when the Austrians used a helicopter on a tether to carry an observer. The infancy of such fundamental inventions has been grievously and unnecessarily prolonged by failure to support them during their inchoate period by useful patents or

by other devices.[5] Similarly with scientific discoveries and laws—it takes years for them to be embodied in numerous inventions, and so to become "great" in an economic sense. Dyson claims that in Physics about thirty years elapse between recognition of a puzzling phenomenon and its explanation, and another thirty years are required to work out its consequences.[6]

A second reason why invention lists downgrade the modern, and hence cannot be used to measure recent invention, is that in such lists, highly subjective, the compiler inclines to favor the famous inventions of the past, whose relative simplicity makes them comprehensible and whose long use makes them familiar—e. g., barbed wire rather than some chemical innovations of today, which few can understand. We can see the past as historians, see it whole, but the present we can see and appreciate only in part, as specialists in some field we may have mastered—unless we resort to elaborate statistical devices, as we shall later.

Another factor which helps account for the deficiency of lists of inventions is the probable tendency to appraise each in comparison with its contemporary inventions, listing the best of each period. Thus the incandescent filament light of Edison and his predecessors is recognized as a difficult invention at that time, as well as outstandingly successful, so it is sure to be listed. A modern invention of electroluminescence may be just as difficult—and just as successful (in the future)—but it is lost sight of amid a swarm of electric inventions being turned out in thousands of laboratories by hundreds of thousands of physicists and engineers. If this principle of selection by preëminence among contemporaries were the only principle followed, the yearly rate of invention-listing would be always the same, could never expand, even for population growth.

Still other approaches have been used by social scientists seeking to measure invention. Hornell Hart,[7] going back thousands of years, has interestingly measured progress in terms of speed of travel, radius of attack, span of bridges, speed of cutting tools, etc., parameters reflecting invention, but not sufficiently numerous to measure invention in general. Schmookler, Markham, Abramovitz, and other economists have measured the increasing productivity of the United States and have attempted to assign a definite share of this to invention.[8] But productivity is affected by many other

factors beside current American inventing—foreign progress, the use and disuse of old inventions, scientific discovery, improvement in quality of goods, introduction of new goods, the efficiency of labor, the supply of capital and land, the discovery and exhaustion of resources, and losses by wars, mistakes, and strikes. From this welter of causative factors it seems unlikely that even the subtlest and most laborious economist could extract and measure the single factor of invention; and if he did he would still have measured only the *use* of all newer inventions, and not the yearly rate of contriving inventions in America, which is our goal. But for what interest it has, our Gross National Product is stated as growing 60 per cent per decade since 1929, and somewhat slower before.[9]

II

If we are to measure American inventing we must follow some different plan, one that will isolate this variable or come near to isolating it, one that will be independent of the subjective judgments of a few individuals, one that will be statistical and scientific, and one that will cope with the besetting yet ineluctable sin of Statistics, which is adding together differing things simply because they are called by the same name. Elephants and clams are both animals, for example, but we must not add them together, unless perhaps in genetic studies. Patents are just such a transforming variable, called by the same name in all countries and even granted under almost the same laws in America today as in 1880, yet differing vastly, and with differing averages over time. The length of the printed patent document has more than doubled since 1907; patents have become increasingly the product of corporation laboratories and assigned to a corporation on issuance—12 per cent in 1885 compared to 61 per cent today; and Sanders[10] finds that about 60 per cent of patented inventions are now worked at one time or another, whereas earlier estimates of use ran far less. Individually patents represent anything from fundamental inventions to mere trifles. Taken in mass their average in different decades and countries is less variable than individually, but still much too variable to be used for measuring invention across time. We must look elsewhere for tolerably inelastic yardsticks for measuring invention.

In order to apply statistics to invention, we must *define* invention, or whatever it is we are counting, choosing a definition that can provide workable statistics and a usable result. Engineers tend to define an invention as a set of ideas legally and economically patentable. Such a definition would return us to the fiasco of patent statistics and would have but small correspondence to economic and social data and problems. It would seem best to define invention much as we do in common speech, viz., all new ideas, big or small, for making new things or for making old things in a new way, such ideas to be totaled by weighing, not counting them, i. e., with full consideration given to the vastly differing importance of inventions. Furthermore, we should add in scientific discoveries in physics, chemistry and metallurgy, the sciences which most directly feed invention and vice versa, and which hence are tied in with its statistics. And for truer naming we should call the result we are measuring Inventive Effort rather than Invention.

How do we usually assess the importance of an economic activity? By the money spent on it, i. e., its total cost—making the assumption that it is worth about what people pay for it—or else by counting the man-hours devoted to it. For example, if we wish to measure the importance from year to year of education, of war, dancing, or anything else difficult to measure, we should probably consider the sums spent on it and/or the number of people involved and the hours or days they were attending to it; and instead of a crude man-hour count we should take account of the value of different peoples' time, since an hour's time by a great professor, for example, brings more of education than an hour of attendance by one kindergarten child. Such standard approaches to statistical inquiry are just what we should use in measuring inventive effort: Let us seek from year to year its cost and its man-hours, weighted according to the quality of the man. We cannot find statistics completely to our point—no one ever can—but we can at least find fair indicia, variables that should be concomitant with the inventing we seek to measure.

Suitable cost figures for organized research and development, mainly or entirely in physical, inventional lines, are available for industrial research from 1920,[11] and for federal funds from 1900.[12] The vast growth of organized invention has latterly bid up the salaries of engineers, chemists, etc. thus employed, so that we lack

a suitable price index for them. But considerable data are available on the size of the professional grade staffs since 1920; hence I have used this measure, with many needed adjustments, in preference to money spent.

But this evidence leaves out the unorganized inventors and the smaller laboratories, as well as those which did not report to the government. We may get at these, along with the larger invention-factories, by counting the numbers of engineers, physicists, chemists and metallurgists from year to year, either by census figures or through the membership of the principal professional societies, or through educational statistics. I have used all these, but principally the leading societies. For education I used the number of Ph. D.'s awarded in Physics and Chemistry and the enrollment of engineering students, in order to assemble data reaching back to 1900 or as far as 1880. The doctorates were lagged 11 years and the engineering students 15, so as to date the men in the midst of their inventive career.

Another means for covering inventions, as well as those discoveries in physics and chemistry which eventuate in invention ten years or more later, is to count the abstracts of papers by Americans, published in *Chemical Abstracts* and the prior *Chemisches Zentralblatt*, in *Physics Abstracts* and its predecessors, and in *Engineering Index* and *Electrical Engineering Abstracts*. On some of these, counts of the American papers can be found, on others sample counts were made, reaching back to ca. 1900 or 1880. The abstracts have the special advantage of being measures of inventive *output*, the first valid ones ever presented, whereas all the rest measure input, inventive effort.

When plotted on ratio chart paper these different indicia of invention gave a series of graphs strikingly straight from beginning to end, save for minor fluctuations, and parallel to each other. Out of nineteen plotted the only exception was in the least important element, from *Electrical Engineering Abstracts*, which rose but 11 per cent per decade (1904-55), reflecting perhaps an exhaustion of the possibilities of progress in that line until the electronic age began showing since 1950. The unanimity of the other graphs is statistically very satisfying. My next step was to combine the eleven most significant graphs, with suitable weighting, into four class totals—the abstracts, professional society members, lagged

counts of students of engineering and physical and chemical doctors, and the professional workers in laboratories public and private. Each of these four subtotal graphs was still straighter and more parallel than before. The last step was to combine these four into one, again with carefully suited weighting; but actually the weighting scarcely mattered, so nearly parallel were the elements. The final result was nearly a bee-line on the ratio chart, indicating growth at a constant rate—except for a small check around 1915 and a slightly slower rise from then until 1950, which might be due to the era of wars. The total rise from 1880 to 1955 was 135-fold, spelling an increase of 91 per cent per decade and 7 per cent per year. Patents to Americans, in the same 75 years, increased only 2½-fold, not even keeping pace with population, which rose threefold.

III

If this measure of the rise of American invention and invention-oriented scientific research can be believed, it demonstrates the relative decline of the patent system. But the reader might well ask if this measure can be true if the counts of patents and of listed important inventions run so counter to my finding. Furthermore, although we might concede a sharp rise in invention, we might be reluctant to believe that it rose 135-fold!

To remove these understandable doubts we might first ask if anything else in America has gone up as much as 135-fold in the last 75 years. Examples are easy to come by: telephones have increased 1160-fold, cigarettes 754-fold, productivity multiplied by population 36-fold, secondary education 68-fold. Mere increase of population should account for one third of our phenomenon, reducing the per capita rise of inventive effort to 38-fold. One may object that our greatest rises are in things like telephones and cigarettes that scarcely existed in 1880, so that their cited growths are almost as if we had divided by zero, a mathematical procedure meaningless and forbidden. True, and indeed we are measuring something of the sort, rising from near zero. Invention today is a product chiefly of laboratories, and in 1880 there was only one tiny invention laboratory in the country: Edison's first, before the erection of his large establishment in Orange in 1886; du Pont's did not come until 1889. Invention is old and science is old, but

scientific inventing in laboratories is a new thing on the face of the earth.

This raises a perhaps valid criticism of my statistics, viz., that what I have been measuring is not just invention, but chiefly the more scientific kind of inventing, and also physical and chemical discovery, and the expression of all three in publications, education, professionalism, and gathering into laboratories. In brief, it might be charged that my measures are largely of literacy and organization in invention, and hence are underrepresentative of the old-style, unscientific, cut-and-try, amateur school of invention which was relatively more important in the early decades of our period. Perhaps so. But on the other hand there is no reason to think this old-style, simpler inventing has declined per capita; it is abundantly, even if not fully, represented in the publications and in the daily work of engineers and other professionals in and out of laboratories. As for the contrasted, modern, scientific kind of inventing and scientific discovery of new bases for invention, it certainly has increased vastly, as my indices show, and I attribute a large part of this increase to the rise of chemistry, pure and applied. The increase is not surprising: the growth in scientific literacy, professional communication, organization, and equipment could be expected to foster invention—that is their *raison d'être*. Again, since the modernizing of inventive method has been a progressive thing, and the laboratory system first became important only forty or fifty years ago, we might expect an upward curve in the graph; instead, its otherwise very straight proportional rise was checked around 1915, as we have seen, just when the laboratories were becoming important.

Another valid criticism is that my index is of Inventive Effort, not invention, and that there is no reason to presume the two have kept pace—that a measured amount of effort has always throughout these 75 years resulted in the same proportion of success, i. e., invention. Indeed, there are reasons to believe we may face diminishing returns. For example, the "easier" inventions might have been made first, leaving the hard ones to labor over now. Again, when inventors and scientists were fewer in the community, they were presumably more selected by competition, whereas today almost any graduate in engineering or science can get a job in a laboratory, perhaps a mathematician or someone else not even

inventive, only presumed useful; there he would inflate our index whether successful at invention or not. Moreover, there are grounds for thinking that the college training and ensuing occupational career of engineers has a stultifying effect upon the inventive faculty, despite the ever-growing need of scientific education.[13] Some say that the laboratories are full of time-wasters. Confirmation for this suspicion is found in my charts, which show only an 84-fold rise in the abstracts, our measure of output (corrected for modern military secrecy), but a 226-fold rise in the remaining measures as weighted, those of input. A fivefold rise in the ratio of subprofessional to professional laboratory employees, 1921-54, suggests a shortage of trained inventors, pieced out from lower levels. On the other hand, had this growing assistance to the inventors been included, it would have made our graphs of input still steeper.

On the other hand, if all these laboratory arrangements and numerous employees do not promote invention, why has so much been spent on them—over ten billions for research and development in 1959 by public and commercial authority? And is it not to be expected that all this material equipment should be helpful to production, and likewise this specialization of function, Spencerian integration, which is the essence of a staffed laboratory as compared with an isolated inventor? If the inventor needs some machining done, or the use of an electron microscope, or the advice of a metallurgist or a patent attorney, or a translation from the Russian, or a search of the world's prior art, the specialists and equipment are right at hand to do it for him, and much more expertly and quickly than he could himself.

But what about the "lowest," least scientific grade of invention, which I admit is not fully represented in my indices, and which was relatively more important in the past than now? Statistics available from the National Association of Suggestion Systems prove that it is still vast. Data from 206 companies and American government departments organizing that activity among their 6,000,000 employees showed they accepted 1000 suggestions each day of the year 1955, rewarding them with an average of $30.22. The estimated savings due to these suggestions amounted to $20,000,000 in the first year of use. To be sure, few of these accepted suggestions were inventions, and almost none is to be

found among the 29,473 patents of 1955. Nevertheless, these suggestions show that a great flood of lowest grade invention continues —and does not depend on patents.

Much of modern invention and invention-oriented research is for military purposes, amounting by cost to about one-half of the organized work. Military use tends to reduce publication, an effect very visible in the statistics of abstracts since 1940, but not in the other indices of invention. It has also reduced patenting: patents on military inventions are few and of little significance, since the government is free to use any it needs, and to pay only what it thinks fit. A considerable number of other patents are likewise of limited importance, since they are held by the government, a university, trade association or a freely licensing corporation or patent pool, instead of being held in the restrictive, private-profit-maximizing manner envisaged by the patent system; and some have been practically invalidated by a court. All in all, such nominal rather than effective patents probably amount to one-third of all, probably a larger fraction than in 1880.

Economists usually—and wrongly—ignore military, governmental invention. Its causes, producing factors, and measurements are inextricably enwrapped with those of civil invention; the same scientific advances, the same technical education, and the same personnel and laboratories serve both war and peace. Also, the inventions which are devised for one constantly come to serve the other, as in the case of aviation, navigation, metallurgy, atomic energy, etc.

IV

Since invention is always a new combination of old elements of technology and science, we should expect an ever swifter proliferation of invention as our stock of combinable elements increases. To be sure, this also makes inventing a more ponderous and learned task, but such problems are met by more of technical education, documentation, and organization in laboratories of cooperating specialists. Inventive Effort, we have seen, appears from our admittedly inaccurate statistics to have increased something like 135-fold in America since 1880, while patents to Americans multiplied only 2½-fold, falling to 2 per cent of their former ratio to Inven-

tive Effort. One would therefore be justified in posing the question: why is so much less of invention being patented today?

Some reasons we have just indicated. The rise of government inventing to half the total (as measured by the expenditures for organized Research and Development), the coming of invention by other non-commercial bodies, and the growing percentage of patents struck down by court decision or opened to compulsory or even free license to competitors, must all decrease the attractiveness of patents. Again, today's corporations, being larger, have less need for patents (even if they have often used them to buttress a monopoly) because they can exploit their inventions on a larger scale, whether patented or not, whereas a small competitor is dependent on patents for any large reward. Another contributive factor might be that enterprisers are more educated scientifically and socially than formerly and therefore less inclined to hold their inventions for themselves by patents and infringement suits; pure scientists and physicians often refuse to take out patents.

A similar falling-off of the patenting rate is visible in all industrial countries, indicating that the causes are to be sought in the nature of contemporary world-wide technics and culture rather than in any accidents of American legislation or administration.

When patents become fewer, for whatever reason, we should certainly expect that they would become better individually (i. e., represent a more far-reaching advance over previous devices and processes) because only the best inventions would be patented. This has certainly happened: American patents have doubled in word length, and the percentage actually worked has risen far above the estimates of former decades, Sanders proving[10] that 60 per cent of the inventions recently patented by Americans are worked at one time or another. This latter point is borne out by the fact that today 61 per cent of patents are assigned on issue, usually to the corporation that hired the inventor, whereas only 12 per cent were assigned in 1885.

In addition to being "better" and used more, today's patents are probably broader in coverage than their predecessors. The expansion of science and of scientific education should enable a modern patent to cover more ground, through a clearer appreciation of the meaning, scope, and possible applications of the principles involved. At the same time, improved science should greatly

help invention and, by improved documentation, assist in the discovery of prior inventions of the same idea, reducing duplication of patents.

Since patents have probably improved in quality and percentage worked, it is evident that the importance of the patent system as a whole has not declined so cataclysmically as the ratio of the number of patents to inventing (to 2 per cent of what is was in the 1880's). How then are we to assess the remaining real importance of the patent system?

One accessible statistical avenue of approach is the valuation set upon royalty income from patents by those corporations which hold the thickest portfolios of them[14] and which also furnish royalty data in their annual reports. Of the 17 largest patent holders recently reporting their royalty income, such income amounted to $1223 per year per patent owned, or 1.6 per cent of the corporations' gross income. If we could take the principal patent holding corporations before 1941 or 1913, when income taxation came in to affect the reporting, and see what valuations they then set upon their patent holdings, we might have a more significant figure than the royalties.

Another possible approach to the measuring of the importance of patents today would combine statistics with guesswork as follows: one may guess that about half, by weight or value, of all invention is below the level of patentability, i. e., excluded either by law or by economics (not seeming worth a patent). Of the remaining upper half of invention, we may guess–based on initial assignment in 1955 of 61 per cent of patents–that a quarter is unorganized while three-fourths is organized in laboratories. Of the organized we know that 52 per cent by cost is non-commercially financed and therefore not patent-motivated; let us assume two-thirds of the commercial inventing to be seeking patents. In the case of unorganized inventing, let us guess it is 90 per cent commercial, with three-quarters of that patent-seeking. Working out the above data and presumptions, we find about 21 per cent of invention to be patent-motivated. Lastly, taking thought of our other evidence, we may estimate that about one-fifth of today's American inventing is considerably motivated by the patent system.

The extent of this relative decline of the patent system may be surprising; but the patent system has probably not declined at all

in absolute terms. There has been some recent falling off in number of patents, but those taken out are qualitatively better and are more used, even if oftener destroyed by the courts. Thus the patent system still performs an important function, especially for the small concern and the free-lance inventor.[15] Nevertheless, my findings prove that patenting has greatly declined relative to the rate of inventing, which has risen enormously. Chiefly responsible for this contrast is the vast efflorescence of invention and research financed by big business and by the federal government, making little use of patents.

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[2] L. Darmstaedter, *Handbuch zur Geschichte der Naturwissenschaften und der Technik* (Berlin, 1908).

[3] C. Streit, *Freedom Against Itself* (New York, 1954), with list of 1012 major inventions, discoveries, and innovations since 1750, with their attributed country, authors and date. Streit's cogent argument for Atlantic Union is not, however, helped by his finding that 97 per cent of the inventions, etc. came from the western countries proposed for Atlantic Union. The other factors beside political freedom that operate for and against invention should not be ignored.

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[9] Calculated from *Economic Report to the President* (1957) in stabilized dollars.

[10] B. S. Sanders and J. Rossman, "The Patent Utilization Study," *Patent Trademark, and Copyright Journal*, I (1957), pp. 74-111.

[11] V. Bush, *Science the Endless Frontier*, (Washington, D.C., 1945), p. 80; Y. Brozen, "The Economic Future of Research and Development," *Industrial Laboratories*, IV (December 1953).

[12] United States National Science Foundation: *Federal Funds for Science, VII, The Federal Research and Development Budget, Fiscal Years 1957-59* (Washington, D.C., 1958).

—, *Scientific Personnel Resources* (Washington, D.C., 1955), Table A-1.

—, *Methodology of Statistics on Research and Development* (Washington, D.C., 1959), *passim.*

[13] Gilfillan, *op. cit.*, pp. 112-14.

[14] *Distribution of Patents Issued to Corporations, 1999-55* (United States Senate Judiciary Committee Study No. 3; 1957), Appendix. *Moody's Industrial Manual* (1957) was used for financial data.

[15] B. S. Sanders, "Variations in Utilization by Different Types of Companies," *Patent, Trademark, and Copyright Journal*, III (1959), pp. 56-60.

AN ECONOMIST TAKES ISSUE

JACOB SCHMOOKLER*

It is with some reluctance that I join issue in public with Colum Gilfillan. All students of invention owe him so much for his masterful and brilliant *Sociology of Invention* that it seems almost ungrateful to hold him wrong in some particular. Yet, since it was at Gilfillan's suggestion that the editors of *Technology and Culture* asked me to criticize his article, I hope I shall be absolved of this sin.

Before discussing Gilfillan's paper, I should like to correct some misconceptions of his about my own work. Specifically, I have never assumed that "the average patent of today goes with the same amount of invention as in, say, 1880." In the first place, *I have never held that patent statistics could be used to compare the level of invention after 1940 with that during the period before*: there is ample evidence that the patent-invention ratio fell after

* Dr. Schmookler, Associate Professor of Economics in the School of Business Administration of the University of Minnesota, has written on the economics and sociology of invention.

1940.[1] In the second place, *for the pre-1940 period, I have held only that the behavior of patent statistics is consistent with what little external evidence exists as to the course of American inventive effort in that era.*[2] After a careful study of Gilfillan's article, I feel compelled to reaffirm this conclusion. I should at the same time call attention, as I have in the past,[3] to a possible downward bias in the data during the pre-1940 era and recommend their use more for periods of intermediate duration than for long periods.

Limitations of time and space preclude any description here of the possible uses of patent statistics. I can merely assert that these data can be used, with caution, of course, and together with any other evidence available, to throw light on (a) temporal changes in aggregate invention, (b) spatial differences in invention, (c) industrial differences in inventive activity, (d) temporal changes in invention by industry, and (e) social, psychological, and economic attributes of inventors and their backers. The interested reader can consult my forthcoming review of the literature just cited for a defense of these claims. Statistics aside, scholars in the field can ill afford to ignore the finest record of American technological progress in existence, the files of the United States Patent Office.

Turning now to Gilfillan's own index of inventive effort, I shall first criticize his article in terms of the claims he advances on behalf of his index, and conclude with what I hope is a more useful alternative description of it. Gilfillan's contentions are, I think, subject to the following serious criticisms: (1) The alleged 135-fold increase in inventive effort from 1880 to 1955 is wholly inconsistent with (a) his admission that the index largely ignores non-professional invention and (b) his contention that invention of the latter sort is relatively as common now as in 1880. (2) The index certainly overstates the rate of growth of invention for the pre-World War II era relative to that of more recent years. (3) The index reflects either (a) a serious misconception or (b) a highly unwarranted assumption about the work of scientists and engineers. (4) His claims that he reckons inventions and inventive effort according to their worth are grossly misleading. And (5) by including scientific effort and discovery in his index he covers phenomena not comprehended by invention as the term is ordinarily construed.

(1) In 1880, Americans filed approximately 21,000 applications for patents on inventions.[4] In the same year, there were approximately 2,000 chemists, assayers, and metallurgists, and 7,100 technical engineers and surveyors.[5] (Engineers and surveyors were not recorded separately in the source.) Since, in 1885 only 12 per cent of all patents were assigned to firms,[6] of which some were undoubtedly produced by supervisors, skilled workers, and independents, it would be generous to assume that these 8,000 or 9,000 scientists and engineers were responsible for as much as 10 per cent of the applications filed in 1880. However, let us assume that, when 'suitably weighted' for quality, the inventive effort of this group actually amounted to *half* the total social inventive effort of the time. In this extreme case, setting the total social inventive effort in 1880 equal to 100, 50 units were contributed by the scientists and engineers, and 50 by all others.

By his own claim, Gilfillan's index essentially measures the inventive effort of scientists and engineers. Hence, if we take his index at face value, the contribution of scientists and engineers rose 135-fold by 1955, or from 50 in 1880 to 6,750 in 1955. If it is further true, as Gilfillan asserts—though hardly anyone else agrees—that the per capita inventive effort of the balance of the population has been sustained throughout the period, then the contribution of this sector rose in proportion to the growth of population, or by a factor of 3.3. The contribution of the rest of the population therefore rose from 50 in 1880 to 165 in 1955.

Taking the 1955 contributions of both groups we get a total social inventive effort of 6,915 (6,750 plus 165) in 1955, compared to 100 in 1880. Instead of a 135-fold increase, we get an increase of 69-fold, or half that claimed by Gilfillan.

If, as is probably more reasonable, the scientists and engineers are credited with only 25 per cent of the 1880 effort, again with quality taken into account, the aggregate increase becomes only 36-fold. And if scientists and engineers are credited with only 10 per cent of the 1880 total, the aggregate increase becomes only 16-fold.

Of course, if everybody else is right in believing that invention in the populace at large has fallen in terms of both quantity and quality, then the increases indicated above would be still smaller.

Gilfillan, I suppose, will concede the point but perhaps argue

that even so, the 16- to 69-fold increases suggested by the foregoing hypothetical calculations are far greater than the growth of patents from 1880 to 1955. Such a response would have merit if anyone ever claimed patents measured comparative effort between those two years. To my knowledge, no one has done so. In 1880, 13,000 patents were granted; in 1955, 30,000. To hold that those numbers indicate the comparative magnitude of inventive effort would be absurd. On the other hand, in 1930, 45,000 patents were granted. To suggest that the 3.5-fold increase from 1880 to 1930 *might* indicate the growth of invention, which is the most I have done, is not absurd. Over the same period population increased only 2.5 times.

(2) From his own description of its behavior it seems clear that his index overstates the pre-1940 rate of invention relative to the post-1940 period. He tells us that it shows a smooth rate of growth, at about 87 per cent per decade, a statement which he qualifies solely by adding that the rate is slightly less after 1915 than before. From this one may infer that the rate of growth after 1940 is substantially like that during the decade or so preceding. If this inference is incorrect, I assume Gilfillan will so inform us.

Yet organized research and development expenditures in the United States stood at about $350 millions in 1940[7] and at about $6 billion in 1955.[8] When these figures are scaled down, say, by the change in average annual earnings of full-time manufacturing workers[9] for the period, instead of a 17-fold increase, the increase becomes a little over 5-fold for the 15-year period. The latter, on a decade basis, amounts to a rate of growth of about 200 per cent, and an annual rate of over 11 per cent.

These crude estimates merely reflect the well-known acceleration of research since the onset of World War II. Beyond question, even after allowance for unorganized invention, the rate of growth of invention after 1940 far exceeded that in the pre-1940 period. Yet no such acceleration appears in Gilfillan's index, if we are to accept, as we must for the present, his own description of it. Either (a) the rate which he shows for the pre-1940 period is too high, or (b) that for the post-1940 period is too low, or (c) both. My guess, and it is only a guess, is that (c) is correct.

(3) Gilfillan has apparently treated the chemists of any given

year on a par with those of any other year. This is likewise true of his treatment of engineers, physicists, etc. He therefore implicitly assumes that members of these professions either (a) spend all their time inventing, or that (b) the fraction of the time spent in inventing is constant within each group. (a), of course, is false. Members of these professions may spend their time *using* received knowledge, *teaching* it, *advancing* it, or doing something largely unrelated to their training. In 1953, a year of relatively high research effort, less than 30 per cent of the scientists and engineers employed by private industry were engaged in research and development.[10] This percentage was probably far higher in 1953 than in any year before World War II. There is absolutely no evidence, and no reason to suppose, that the fraction of time spent on invention by scientists and engineers has been constant or nearly so.

(4) The foregoing leads to the next point. Gilfillan promises to give "full consideration . . . to the vastly differing importance of inventions." Obviously, however, he treated each item in a given abstract as the equivalent for any other item in it. To have done substantially otherwise would clearly have been impractical for an individual to attempt. This, of course, is the same kind of deficiency with which he taxes patent statistics—the treatment of unequal items on a basis of parity. Moreover, if he went so far as to differentiate between abstracts, one would like to know how and why.

Again, one would like to know why he supposes that the same ratios between inventing, publishing, and abstracting prevailed throughout the period.

In similar vein, while he promises to reckon invention according to its "cost," in the very next paragraph he abandons cost for data on the number of professionals employed. And though he promises to weigh the hours spent in inventing "according to the quality of the man," he has apparently counted all Ph.D.'s in science as, say, 2, engineers as 1, and the rest of us as 0, or something of the sort. Leaving the engineers to look out for themselves, I should like to protest on behalf of the ciphers. But joking aside, this sort of procedure hardly lives up to its advance billing.

On the other hand, independently of the merits of his index, if Gilfillan has developed a sound basis for weighting the different

professions according to the average value of the inventive input per man, and the different abstracting services according to the average value of the inventive output disclosed per abstract, he has accomplished a great deal. We shall all be benefited when he discloses and adequately defends his procedure.

However, I am inclined to believe that in substantially ignoring non-professionals, Gilfillan fails to recognize that the margin of superiority of inventions made by scientists and engineers over those made by skilled workers and supervisors was probably far smaller in 1880 than in 1955. Since industrial technology was simpler then, it was more susceptible to improvement by skilled workers and supervisors than seems the case today. At the same time, since science and engineering were far less developed in 1880 than now, it may be that training in them did less to facilitate invention then than now. These considerations suggest the desirability of including more occupations in his index, with the weights shifting over time increasingly in favor of the scientists and engineers.

(5) Finally, Gilfillan has elected to include in his index not only inventors and inventions as ordinarily construed, but also scientists and scientific discoveries. There is, of course, a growing affinity between science and invention, and there may be some justification for considering them as a single phenomenon.[11] While I doubt that such treatment is ordinarily useful, if only because of the past and present institutional differences associated with the two phenomena, it is perfectly clear that the behavior of a measure of the two jointly can hardly be used to assess the validity of another measure, i. e., patent statistics, which purports to measure only one of them separately.

Some idea of the difference between invention in the ordinary sense and invention in the sense in which Gilfillan uses it may be gained from the fact that where he uses a count of articles listed in *Chemical Abstracts* to construct his index of inventive effort (output?), the same data have been used by Fisher as an index of basic research.[12]

Of the five criticisms listed above, (1), (4), and (5) pertain not to his index but to the claims Gilfillan advances on its behalf. The increase in invention was smaller by far than he thought, his index does not really weigh inventions and inventors according to their

individual value, and it covers a phenomenon different from invention as ordinarily conceived. By renaming it an index of scientific and engineering effort, we may find it quite useful. On the other hand, we will still be troubled by the failure of the index to show a higher rate of growth after 1940 than before and by the various constancies he has implicitly assumed. These deficiencies, unfortunately, may not be remediable. But I should hope and expect that when conceived of as an index of scientific and engineering effort, Gilfillan's index may yet prove valuable. When his completed work is published, we should be in a better position to judge.

REFERENCES

[1] Cf. my "The Level of Inventive Activity," *Review of Economics and Statistics* (May, 1954), footnote 8. Cf. also my forthcoming "A Critique of Patent Statistics and a Review of the Literature."

[2] *Ibid.*

[3] *Ibid.* As to the pre-1940 period, "We must suspect that more recent patent-statistics are under-weighted relative to earlier ones, so that intermediate period comparisons are safer than longer range comparisons." Again, in "The Level of Inventive Activity": "We can draw no firm conclusions about the reliability of patent applications as a measure of inventive activity for the period 1869-1938 as a whole. . . . Patent application statistics are useful as an index of change in aggregate inventive activity when the periods compared are not too far apart." p. 185.

[4] *Historical Statistics of the United States*, Series P. 182, less an assumed seven or eight hundred patent applications filed by foreigners.

[5] U. S. Census, *Comparative Occupational Statistics of the United States, 1870-1940*, Table 8.

[6] Alfred B. Stafford, *Trends of Invention in Material Culture*, University of Chicago Ph. D. thesis (microfilm, 1950), Ch. VII.

[7] Philip N. Powers, "The Changing Manpower Picture," *Scientific Monthly*, LXX (1950), pp. 165-171.

[8] Average of estimates for 1954-55 and 1955-56 given in National Science Foundation, *Reviews of Data on Research and Development* (Dec. 1959), Table 1.

[9] Taken as roughly indicative of the course of salaries for scientists and engineers.

[10] National Science Foundation, *Science and Engineering in American Industry, Final Report on a 1953-1954 Survey*, p. 3.

[11] Cf. Bernard Barber, *Science and the Social Order* (Glencoe, 1952), p. 194.

[12] J. C. Fisher, *Who Does Basic Research in Industry?* General Electric Research Laboratory, Report No. 58-RL-2075 (Schenectady, Sept., 1958).

A PATENT ATTORNEY TAKES ISSUE

I. JORDAN KUNIK*

Ever since Gilfillan wrote his essay on "The Decline of The Patenting Rate, and Recommendations," [1] he has been distressed by the seeming recalcitrance of the patenting rate to parallel the curve of population growth. At that time (1935) he even suggested, as one of the causes of patent decline, that:

> "The native ability of the American people has been declining, thru dysgenics and immigration, latterly chiefly of the poorer classes, come mostly for purely economic reasons. It is certain that for several generations past the stupid have been breeding at a much higher net rate than those with native intelligence, and that as to acquired intelligence, i. e., education and other such advantages, socially transmissible, the distribution of child-raising has been equally perverse. . . ." [2]

Later on in the same article, he expressed his bias by describing the patent system as entrusting the whole aim of encouraging the use of good inventions to "an attempted private monopoly" while paying tribute to the Russian system of putting approved inventions to work.[3] In many of his writings [4] he has espoused and preached a deterministic evolutionary doctrine concerning the nature of invention. Such an hypothesis postulates the virtual inevitability of the successive steps of invention wherefore no one is properly entitled to obtain a legal monopoly (patent) on any area of technology. He concludes by saying that "we need a radical reconsideration of the whole patent system, which is decaying not only in America but in other countries; . . . ."

One may admire a man for his prolonged loyalty to his doctoral thesis, *The Sociology of Invention* (1935), but it is important to keep in mind Gilfillan's prejudice in order to understand his transparent wish, despite tangential disavowals, for the demise of the patent system. Since the patent system still appears quite viable in our present economy, Gilfillan doesn't give up. Should he truly

* Mr. Kunik, a patent lawyer in New York City, is active in many scientific and engineering societies and is deeply interested in the relations of patents to the inventive process.

feel that the patent system is in decline, he might well have let it expire unnoticed, but somehow he feels compelled to administer a few last kicks to the die-hard invalid, even if it means reiterating vague and improper assumptions and leaping to unsupported conclusions.

In setting out to prove his prepensed notion that there is a significance in a supposed negative correlation between the rate of invention increase and the patenting rate, Gilfillan builds up a picture of purported increase in inventions by assembling an assorted conglomeration of factors, including such things as cost figures for research and development, numbers of engineers, physicists, chemists and metallurgists, census figures of professional societies, numbers of science Ph. D.'s awarded, abstracts of scientific papers, and general population growth. "Suitable weighting" is applied, and, *mirabile dictu*, the final result indicates "growth at a constant rate" of invention as against a considerably slower growth in the patenting rate. His triumphant conclusion is that "the decline of the patent system" has been demonstrated.

This will not stand up in court, as the saying goes. Gilfillan has blithely walked into a semantic net and has become entangled with the multifarious ramifications of the word "invention" and its many conceptual and operational variations. Furthermore, he has failed to recognize that the concept and process of invention are not amenable to statistical "weighting." He declares that the combination of the factors that he has selected shows an increase in "Inventive Effort" of 135-fold in America since 1880, while patents to Americans multiplied only 2½-fold.

He has assumed without proof or without any other logical demonstration that his putative "Inventive Effort" is either equivalent to invention or productive of invention. It would appear that only a small fraction of what he calls inventive effort has been or is being directed to actual invention. It may rather be found that it is concerned in the main with mere low level innovation, improvisation, or adaptation of previously known arts to new requirements, even though these activities may be supported by governmental and private research and development funds and are engaged in by technologists ranging from Ph. D.'s to bench mechanics.

For Gilfillan to establish any validity for his thesis he must

distill from his broad conglomerate mass of dollars, man-hours, Ph. D.'s, professional society rosters, and abstracts of scientific papers a more realistic representation of energy devoted to invention, innovation, and improvisation. One cannot stop there. A further extraction must be made to determine how much of this "inventive" activity is devoted–if that is possible–to *patentable invention* which meets the standards established or recognized by the Patent Office and by Federal Court Judges. It becomes evident to anyone familiar with the operations of private, industrial, and governmental research and development and with the requirements of patent law that the energies devoted to producing patentable inventions are clearly but a small fraction of the larger "Inventive Effort" postulated by Gilfillan.

The legal standards of invention that must be met for patentability include novelty over the prior state of the art. Mere novelty, however, is not sufficient; it must rise to an inventive level higher than that manifested by the mechanic having ordinary skill in the art.

This very requirement of a high standard of novelty imposes upon each new invention proferred for patenting the necessity for proving to the Patent Office that a significant advance has been made in the art. As time moves on and more inventions and patents accumulate, the patent applicant has an increasingly difficult job of "overcoming the prior art." In other words, the granting of new patents represents an ever-increasing bar to the granting of succeeding patents in the same field.

It is particularly important to recognize that the dynamics of the patent system are such that its continued operation militates against the granting of new patents, since what is known cannot be patented or "re-patented." A geometrically rising volume of "inventing for patenting effort" is necessary to maintain arithmetical increments of actual patents granted. To expect the patenting rate to rise in parallel with the putative rise of inventive effort is arrant nonsense since it betrays an ignorance not only of the patent system but also of the dynamics, objectives, and conditions of mid-twentieth century research and development. The patent applicant must prove his case over the strong opposition of the Patent Office while the proliferation of the population merely requires a marriage license, if that. Gilfillan has failed to take into

account these dynamic differences that undermine the illogical and incompatible equations that form the basis of his argument.

Gilfillan has never, to my knowledge, made an operational investigation as to the verity of his statement: "Invention today is a product chiefly of laboratories. . . ." He ignores the findings to the contrary of Dr. Schmookler in his article on "Inventors Past and Present,"[5] where the demise of the independent inventor is exposed as a canard. Gilfillan might also have sought enlightenment on this same score from *The Sources of Invention*, by John Jewkes *et al.*,[6] wherein the independent inventor is shown to play a considerably larger and more important role than had been admitted or understood by most speculative writers on the subject.

In explaining a putative decline in the patenting rate, Gilfillan indicates in one place that ". . . today's corporations, being larger, have less need for patents . . ." for various reasons. The implication here is that the large corporations are, or have been, renouncing their patents, but this does not square with his mention elsewhere of the fact that 61 per cent of the patents are now being assigned to corporations and that a large proportion of them are worked. Having buttered his argument on both sides, his reasoning becomes somewhat sticky.

Gilfillan also implies that patents are odious to "pure scientists." Professors Edwin H. Armstrong (frequency modulation) and Norbert Wiener (cybernetics) have taken out patents as have such Nobel Laureates as Selman Waksman (streptomycin), Ernest O. Lawrence (cyclotron), Enrico Fermi (radioactivity), William Shockley (transistor), Irving Langmuir (surface chemistry), Harold C. Urey (chemistry), Glenn T. Seaborg (chemistry), and others. Pure scientists for the most part do not work generally in fields that are productive of patentable invention. When they discover or elucidate certain new aspects of natural phenomena, this subject matter is not patentable by legal definition. As for physicians, let him tell us how many of them, numerically and proportionately, are engaged in inventive efforts. In actual fact their number is so small that their significance is minimal.

Furthermore, if Gilfillan will take the trouble of interviewing patent lawyers and their clients, he will find that those who seek patents are "more educated scientifically and socially than formerly" and that they are clearly desirous of reaping the fruits of their ingenuity.

Gilfillan sprinkles his article with such phrases as "Why is so much less of invention being patented today?"; "When patents become fewer . . ."; "It has also reduced patenting." He cites the fact of 29,473 patents having been issued in 1955 as supporting these allegations.

Referring to the Annual Report of the United States Commissioner of Patents for the fiscal year of 1958, one finds a recapitulation of issued patents for the previous twenty fiscal years.[7] It is significant to note that after 1955 the fiscal year tabulation is:

1956	40,465 patents
1957	44,819 patents
1958	43,407 patents

Making my own calculations for the fiscal year of 1959 by referring to the records printed in the Official Gazettes of the United States Patent Office, I find that between July 1, 1958 and June 30, 1959, well over 51,000 patents were issued. The New York *Times* (January 2, 1960) reports that well over 52,000 patents were issued for the calendar year of 1959. In the course of four years this amounts to a substantial arithmetical as well as percentage increase in the patenting rate.

Even as a special pleader for the patent system, I would not interpret this recent spectacular rise in the patenting rate as necessarily justifying the patent system any more than the scrapping of the system would be justified if the patenting rate remained on a plateau.

The patent system surely is not sacrosanct, but it is only elementary to require that critics be logical and accurate in their arguments. Since Gilfillan does not demonstrate how his selected statistical factors are even evidential of the production of "inventions," much less of *patentable invention*, he fails to make out a coherent case for a negative correlation with the patenting rate, even allowing him the comfort of his figure selected for the fiscal year of 1955.

He has postulated a sharply rising rate of purported inventive effort based upon dubiously founded selected factors, and since the hard figures of issued patents do not follow his curve, he leaps to an argumentative conclusion which is in accord with his long ingrained predilection against the patent system.

If the patent issue figures of the last four years are any indication, the patent system is still viable and fulfilling an important function in our industrial economy. As Jewkes *et al* have discovered through their operational investigations, many significant inventions have been stimulated by the patent system, as a result of which many new and important industries have grown and prospered. Investment in new enterprises based upon patents has been encouraged, and new products have been made widely available for the enjoyment of ever increasing numbers of people and for the easing of their burdens. The patent system can claim credit for being an important factor in our modern industrial economy whose material benefits not many a modern Thoreau would want to do without. If invention is an inevitable evolutionary process, it is pertinent to ask why there has been little industrial or even "inventive" progress in those countries which have weak patent systems, or none at all. It cannot be presumed that the mental capacity and ingenuity of people in India, China, Egypt, Spain, Central and South America are any less than those of the inhabitants of England, Germany, Japan, and the United States. There may not be a proved causal nexus between a strong patent system, on the one hand, and a high degree of industrialization and of wide distribution of consumer goods on the other, but there is sufficient suggestiveness in these simultaneously occurring phenomena to merit a disciplined economic and statistical investigation which would support logically founded evaluations and conclusions as to their relationship.

Such men as Schmookler, as well as Prof. Daniel Hamberg of the University of Maryland, who on September 24, 1959 submitted an excellent memorandum on the subject of patents to the Joint Economic Committee of the United States Congress, are beginning now to make such hard-headed investigations. Moreover, in 1949, the Patent, Trademark and Copyright Foundation was founded at the George Washington University, Washington, D. C., to initiate and support continuing operational researches into the patent system; as its findings are being accumulated and subjected to strict analysis, we may expect a much more valid appraisal of the patent system than has heretofore been proferred by various speculative writers who have relied mainly upon *a priori* notions and who have had various economic, philosophical, or personal

axes to grind. Gilfillan's methods of analysis are not only fallacious but they also are incompetent for the job.

REFERENCES

[1] *Journal of the Patent Office Society*, XIX (1935), pp. 216-17
[2] *Ibid.*, pp. 218-19.
[3] *Ibid.*, pp. 226, 227.
[4] See Gilfillan's chapters in *Technological Trends and National Policy* (United States Government Printing Office, 1937) and his "The Root of Patents, or Squaring Patents by Their Roots," *Journal of the Patent Office Society*, XXXI (1949), pp. 611-12.
[5] Jacob Schmookler, "Inventors Past and Present," *Review of Economics and Statistics* (August 1957), pp. 321-333.
[6] J. Jewkes, D. Sawers, R. Stillerman, *The Sources of Invention* (London, 1958).
[7] *Journal of the Patent Office Society*, XLI (May 1959), pp. 317-18.

DR. GILFILLAN'S REPLY

Since Professor Schmookler and Mr. Kunik have raised quite different objections, I must answer them separately, starting with Schmookler, who is beginning to recognize that patents can nowise be used as measures of inventive activity over the years.* He is right, of course, in stating that they can be used intelligently for certain purposes other than measuring inventive trend, and I have so used them myself in other papers.

Schmookler's first criticism is that my index "essentially measures the inventive effort of scientists and engineers," registering that their contributions rose 135-fold. But the index is not just that: all kinds of inventions and their creators are included, but scientists and engineers increasingly dominate the field. Nor can I agree with Schmookler's statement that "everybody else" believes that per capita invention by the populace at large has declined, for I have

* Professor Schmookler writes: "Dr. Gilfillan misinterprets my position. I call his attention to the second paragraph of my comment in which I state that "the behavior of patent statistics is consistent with what little exernal evidence exists as to the course of American inventive effort" in the pre-1940 era, and to a related set of statements in the third paragraph of my comment. These views, moreover, are substantially the same as those I have expressed in the past—in my Master's thesis in 1947, in my Ph.D. thesis in 1951, in the articles cited in footnote 3, and elsewhere.

never heard that idea put forth by anyone but Schmookler. Of course the *relative* importance of such amateur inventors has declined, but does it follow that their absolute contribution has also gone down? Amateur inventors today have much more education, especially in physical science, than their predecessors; they handle autos and electricity more familiarly than their grandfathers did horses; they have more leisure time and spare capital—why should they invent less? More likely they invent more. The theory that a decline in amateur per capita inventing has counter-balanced the unquestionable swift rise in professional inventing has no statistical backing; hence it cannot serve to minimize the great rise in inventing shown by my figures.

Schmookler's second major objection is that my data do not reflect a great growth of invention from about 1940. True, for an upward turn at that date is just not to be found in any of the 15 variables plotted, except for a temporary jump in organized research and development expenditures in stable dollars, which disappears if the graphs be smoothed over half a dozen years. In these "historigrams," as Ogburn and I found with some others, the progress of some kinds of culture continues uninterruptedly through war periods, as if the wars were not there. Doubtless war hinders some aspects but helps in others. Of course research has been accelerating since the late war, but it has been rising at the same rate ever since 1880, except for the slight check about 1900-1920.

An important problem is raised by Professor Schmookler's third objection: "There is absolutely no evidence, and no reason to suppose, that the fraction of time spent on invention by scientists and engineers has been constant or nearly so." The only data I can find[1] show that the ratio of the professionals in organized research to the number in the major societies, weighted to deflate the engineers, rose from 17 to 52 per cent between 1920 and 1941, then fell to 31 per cent in 1946 and closed at 58 per cent. If one takes the unweighted count, mostly engineers, the number in research and development rose from 28 to 89 per cent, then declined to 46 and closed at 73 per cent. The degree of accuracy which Schmookler demands in this factor is apparently unattainable; but I see little disturbing in that. Those members of the pertinent professions probably had something to do with invention in the

decades before they joined the laboratories, whether they were working in business firms, or doing research in pure science, or designing engineering structures, or educating future inventors. Our index of society members is only 1 of 5, with a weight of 2/11 since 1920, and its graph is quite parallel to the rest, both before and after the start of laboratory statistics in 1920.

In the same years these professional-grade workers had come to be aided by more and more of sub-professionals, rising in the laboratories from 35 per 100 professionals in 1921, to 180 in 1953-54. This important growing supplement is not reflected in our Index; if it were included, along with the previous disregarded factor about which Schmookler inquired, the upward slope of Inventive Effort would be still steeper.

Schmookler's fourth objection, that I promised "full consideration" of the differing weights of inventions, catches me on a rash word, "full." But I have given much consideration to that point by taking so many factors into account. For example, use of the abstracts constitutes a rough weighting: an unimportant gadget might get one paragraph in only one article, whereas an important invention, like the idea of using isotopes as tracers, occupies dozens of whole articles. Likewise in the counts of personnel; important discoveries and inventions usually occupy many men, who are more likely to be physical and chemical Doctors rather than quondam engineering students, and who would be rewarded with high salaries from the research and development funds graphed. Objection is next raised that having prepared these last graphs I then replaced them by a count of professional-grade personnel for constitution of the index. The funds served the purpose of checking and verifying the other graph utilized. Both were quite straight, but funds rose faster; I replaced this by number of professional personnel because I thought their salaries had gone up faster than the retail price level used to deflate the funds: thus I took the gentler rise as the truer.

The elaborate weighting system will, I hope, win approval of scholars. But whether right or wrong it matters little because, as I said, the graphs to be combined were nearly parallel; if fully parallel the weighting would have mattered not a whit. Actually I weighted a chemist as three and a physicist as six engineers, and chemical and physical Doctors as 150 and 187 engineering students,

in order that engineering should not overpower the picture, though it still weighed 65-100 per cent in 1880 and 34-50 per cent today. The outsider to these three professions need not be alarmed—he is not multiplied by 0, but allowed his due chance in the counts of laboratory budgets and abstracts, if he figures there. To be sure he is not granted his full due in the counts of professional men; the index admittedly does not sufficiently represent the lowest grades of invention.

I can only answer Professor Schmookler's fifth objection, that science should hardly be included in an attempt to measure invention, by pointing out, as I did, that the two are inseparable both in the statistics available and in the functioning inter-relationship. Science both old and new is a principal means of inventing—not all sciences, to be sure, but physics, chemistry, and metallurgy.

I am sure that I need not remind Professor Schmookler, not to mention Mr. Kunik, of the great difference between an index and an average. A valid index may be based on a small and sporadic sampling from a total "universe," or even on no sampling of it at all. For example, Massachusetts statistics disregard Connecticut; yet the indices of one state would be equally valid for another where the two states are known to be similar. An index can ignore a factor without falsifying or neglecting that factor, if only it be responsive to something else which varies in the same direction and degree as that factor. Thus in preparing an index, representativeness, or a logical basis, is not a necessity. Nevertheless, it is desirable to base our index on the most logical and pertinent material rather than on the congruent adventitious. Desirable, I repeat, but not necessary. What matters most about an index is not necessarily what it is based on, but the competence and thoroughness of the statistician, the soundness of his reasoning, and how well his statistics score on such tests as internal consistency, use of proper sources, technical devices, etc.

Professor Schmookler has long worked on patent and economic statistics, but he has never attempted seriously to apply these to the measurement of American dated invention, the object of my paper. He knows more economic statistics than I, and perhaps this inclines him to refinements not justified by shaky basic patent data and makes him suspicious of statistics which involve big probable errors, as here. In social science no statistical conclusions

ever have less than a 1 per cent error, and usually we must be satisfied if the first digit is correct.

Mr. Kunik, a patent attorney, relies less on statistics than does Schmookler. Like Jewkes, whose book[2] he acclaims, Kunik avoids statistics by choosing definitions of invention that make all statistics impossible. Invention is defined by Jewkes as an idea seen as the basis of "an important invention," while Kunik demands an article on *patentable* invention, the same to be defined with rising stringency as the years pass, so that invention thus defined would keep better step with patents. Since there are no statistics on patentable invention, scientific consideration of the subject would be impossible if we followed Kunik's ideas. I gave to "invention" a definition suitable for statistical economic reasoning; Kunik's attempt to substitute a definition drawn from patent law obfuscates the issue.

To my way of thinking, Mr. Kunik ignores the whole argument of my paper—that American invention has not been heretofore measured, that it needs to be, and that this can be done by a suitable definition and the statistical procedures I have employed. Instead he seems to indulge in offhand denials that my various indicia reflect invention.

Kunik's questioning of my indicia raises some questions of my own:

Those abstracts in *Engineering Index* and *Chemical Abstracts*—what are they about? Buddhist mythology, maybe? And all those men training in physics, chemistry, and engineering, and joining the professional societies—why do they make such efforts? Could it be that they are interested in the latest progress in those fields with the idea they might themselves father some advances? Finally, what about all those people in the laboratories—how are they spending their time? If they are playing pinochle or otherwise wasting their time, then Kunik is right and Schmookler gets some support; but if they are inventing, or doing scientific, janitorial, or any other work that facilitates invention as defined in the article, then I am right, and we have here a suitable element for an index of inventive effort.* The question at issue is thus simply:

* Mr. Kunik writes: "The abstracts in *Engineering Index* and *Chemical Abstracts*, which seem to awe Gilfillan, do not represent a purported rise in inventive effort. If he were actually to read many of the papers on which these

what are all the scientific, engineering, and other laboratory personnel doing now, how much inventing were their ancestors doing in 1880, and what is the ratio of then to now?

On only one point does Kunik adduce definite, statistical evidence; namely that patents have been running some 13,000 a year higher than in 1955, which just happened to have been chosen as the latest, the "round-up" date, for all my statistics. But he does not mention the 15,000 reduction of pending cases accomplished in these latest years. The inference of the *relative* decline of patents in importance to invention is obvious from my data.* But nowhere in this article do I claim that the patent system has declined *absolutely*, nor that it is decadent, useless, malfunctioning,

abstracts are based, he would surely find that only a small quantity of them can honestly be said to further their particular arts. And a still smaller number is devoted to *patentable* invention. The writers of a large proportion of scientific and engineering papers may well have used their time more advantageously playing pinochle, as many industrial research directors and chairmen of university and college science departments have testified.

"The cumulative mass of abstracts is clearly not a valid factor in supporting Gilfillan's argument. Only a small proportion of engineers and scientists is engaged in true inventive effort; most of them are involved in exploring various ramifications of what is already known, or in routine analysis and quality control which are hardly creative, though necessary to maintenance of industrial production. This is amply corroborated in the "Report of the Conference on Research Goals," sponsored by the National Science Foundation, held on December 3-4, 1959, at Worcester Polytechnic Institute, Massachusetts.

"Gilfillan's structure collapses because he is attempting to make a qualitative measurement by quantitative means. The operational concepts of invention—and of patentable invention—categorically demand that qualitative evaluations must be made of *specific contributions* to science and technology. Such judgments cannot be made by counting numbers of self-serving, unilateral assertions in published papers, nor by adding up numbers of Ph. D.'s, scientific rosters, and dollars. Until a truly qualitative evaluation of inventive effort is made and an acceptable quantitative representation is worked out on such a finding—and this may be possible by rigorous and honest operational research and scholarship—then, and only then, will it be valid to find a positive or negative correlation with the patenting rate which has been recorded quantitatively throughout the years."

Dr. Gilfillan's rejoinder: "Of course many papers and men are of low quality, or duplicative, or unrelated to invention. This matters not, unless the *percentage* of such has varied over the long run. Moreover, duplication is a useful trait insofar as it gives more weight to the more important inventions and discoveries."

* Mr. Kunik writes: "Gilfillan acknowledges my point by offhandedly admitting that patents now issue at a rate some 13,000 a year higher than in 1955. He neglects to indicate, however, that this figure selected by him represents a not inconsiderable 44 per cent *rise* in the patenting rate since his carefully selected 'round-up' year of 1955. Actually the numerical and percentage increases are con-

or otherwise ignominious. And even if it were true that I am biased against it, might one not suggest that a patent attorney is biased *for* it?

Although somewhat extraneous to the argument, I am interested in one of Kunik's charges: "It cannot be presumed that the mental capacity and ingenuity of the people of India . . . Egypt . . . Central America . . . are any less than those of the inhabitants of . . . Germany." Yea, verily, it cannot be so presumed if one is bound by the right theology. But if one were interested in evidence, from patents, treated with statistical precautions, one could find in an old article of mine[3] that India made per capita one superior patented invention for each 2300 or so by Germans, most of India's being by the handful of Europeans there, while a Mexican had 1/230th of the chance of a German.

The rest of Kunik's reply consists of attacks on my supposed ideas published elsewhere, and how the fur does fly, *my* fur! But since it is all beside the point of the thesis supposedly under consideration, we may just let it pass as sound and fury signifying

siderably greater than that. As to the reduction of 15,000 pending applications in the Patent Office, this was accomplished by the hiring of several hundred additional patent Examiners who have been working into a large accumulated backlog of pending applications. Gilfillan has misleadingly implied that this reduction has something to do with a supposed lowering of the patenting rate. In actuality, the *application* rate has ranged roughly between 73,000 and 78,000 per year since 1954. If his attribution of the reduction of 15,000 *pending* cases to a supposed lowering patenting rate was not intentional, then his carelessness is inexcusable.

"The exact figures on which I have based the foregoing statements are published in the Commissioner of Patents' Report for the fiscal year 1958 (*Journal of the Patent Office Society*, XLI [May 1959]). I am not a statistician, but I refuse to take instruction from one who is considerably less than careful with raw data. Certainly, this deliberate obfuscation by Gilfillan throws a heavy cloud upon the statistical techniques alluded to in his principal paper."

Dr. Gilfillan rejoins: "As to my 1955 patent figure being too low, even if Kunik's later largest figure were substituted, it would make practically no difference for my final conclusions. Invention advanced in 1955-59 as well as patents. Since my statistical accuracy is impugned, I should point out that in comparing patenting to inventive effort I specified patents to Americans only; and in my book I use a three-year moving average, which raised the 1955 figure 12 per cent. Also, remember that the reduction of the backlog inflates the later reckoning. Furthermore, it is unnecessary to consider patent applications. Finally, practitioners of the statistical art know that it is not feasible to bring and keep a large series of graphs up to date and that it is customary to use and to terminate statistical series on quinquennial years."

nothing but a scratched ego. I will even supply one grave charge he overlooked: Gilfillan rhymes only with villain! *

REFERENCES

[1] United States National Science Foundation, *Scientific Personnel Resources* (1955), p. 15 and Chart 5. It is further stated that 47 per cent of physicists and 45 per cent of chemists were in research in 1951.

[2] Richard Stillerman, one of Jewkes' co-authors, did excellent work in compiling the history of a large list of inventions of the last sixty years, chosen to minimize the modern work of the laboratories. Cf. my review in *Current Economic Comment* (February 1959).

[3] *Journal of the Patent Office Society*, XII (1930), pp. 259-67, and S. C. Gilfillan, *Sociology of Invention* (Chicago, 1935), p. 85.

* Mr. Kunik states: "It is elementary in law that evidence of former unrelated crimes cannot be introduced against a defendant in a criminal trial. Such former crimes can be adduced, however, when their cumulative pattern shows a tendency or proclivity toward committing the particular offense that is the subject of the proceedings at hand. Concomitantly, when Gilfillan does strange things with raw data and makes admitted guesses while claiming scientific objectivity, it is pertinent to look to his other writings to find clues to his peculiar ratiocination. Since he himself takes every opportunity to cite his own previous works as authority, he cannot deny me that same privilege. I have taken the trouble of reading those Gilfillan papers cited in his footnotes and elsewhere, and I find that they illumine the processes by which his conclusions are reached. Those other writings, produced in maturity, are extremely relevant in revealing that his present selection and handling of data are deliberately calculated to force his conclusions to fit his hypotheses. Since the thesis under consideration falls upon its faulty statistics, rhyme in the end cannot be substituted for reason."

Dr. Gilfillan responds: "As to my making 'admitted guesses while claiming scientific objectivity,' what is the business of a weather forecaster? There is nothing unscientific about uncertainty and a range of probable error."

The Legend of Eli Whitney and Interchangeable Parts

ROBERT S. WOODBURY*

IN SOME LEGENDS the story is such that from its very nature we can never establish its truth or falsity; in others patient historical work—usually external to the legend—can ascertain whether the events actually happened or not. The legend of Eli Whitney's part in interchangeable manufacture is, however, unique in that the clues and even much of the evidence for its refutation are part of the legend as customarily recited. It is also unique in that the legend is not merely a popular one nor even a story given "authority" by inclusion in conventional textbooks. This legend has been retold at least twice with all the paraphernalia of historical scholarship—footnotes, elaborate bibliography, discussion of the sources, and even use of archival material.[1] But in both cases we find the same failure to evaluate the evidence critically, to follow leads to other sources, and to question basic presuppositions. These same faults extend back to the origins of the legend.

Poking back into the beginnings of this legend, one finds evidence to show that it was at least partially created consciously by its hero and uncritically accepted by most of his contemporaries.[2] The *editio princeps* of the legend is equally uncritical; in fact it is frankly an *apologia pro vita sua*. In his *Memoir of Eli Whitney*, Denison Olmstead[3] gives us most of the elements of the legend

* Associate Professor of the History of Technology at Massachusetts Institute of Technology, Professor Woodbury has published several long monographs on the history of machine tools.

The author wishes to acknowledge the research assistance of Dr. Edith Hoffman, now of the University of Connecticut, and a grant from the Wilkie Foundation which made this study possible.

This paper was presented at a joint session of the Society for the History of Technology and Section L of the American Association for the Advancement of Science in December 1959.

and claims to have based his account upon conversations with those who knew Whitney, as well as upon examination of his correspondence and Miller's. Yet Blake writing in 1887 said: ". . . there have not been wanting persons who have endeavored to take from Mr. Whitney the credit of originating the uniformity system and making it a great practical success at the beginning of this century, thus leading in the van of progress of the mechanical arts, and laying the foundations for the enormous industry development of the nineteenth century."[4] Evidently some of his contemporaries were not taken in by Whitney's claims, but the scholars have not asked either who these other inventors were or what their contributions may have been. Let us examine the principal parts of this legend in some detail.

I

The Contract

Whitney's contract of June 14, 1798 to manufacture arms for the Federal Government is the focus of a number of elements of our legend. His motives in this undertaking have been interpreted as those of a prudent businessman doing his patriotic duty and as those of a genius anxious to put into execution a new scheme of manufacture for the good of his country in a time of crisis. His actual motives were quite different.

In 1798 Miller and Whitney had lost all their suits to obtain their cotton gin patent rights in the courts of the South. What little legal merits these decisions had, stemmed from a defect in the Patent Law of 1793; clearly nothing further could be done until Congress corrected this defect. The efforts of Whitney and others did not finally result in a new patent law until 1800. The intervening years could be seen as a lull in the affairs of Miller and Whitney. But Whitney could hardly look forward to any relaxation, for their financial affairs were in desperate straits. Every source of credit had been exhausted by both partners.[5] Certainly Whitney himself was on the verge of a nervous breakdown. Although some have tried to find in this situation a frustrated love for Catherine Greene, a more careful reading of his letter of October 7, 1797 to Miller indicates rather that Whitney's high hopes of financial security, respected position, and prestige have

not only come crashing to the ground, but the disgrace of bankruptcy is staring him in the face. All that winter of 1797-98 Whitney brooded alone, half-heartedly carrying on the affairs of Miller and Whitney. He shut himself off from all his old friends and even unjustly accused his partner and friend Miller.

Whitney needed a new opportunity—any opportunity. But, more important, he needed credit—credit to save Miller and Whitney from bankruptcy, credit to enable him to fight for his rightful profit and for his good name lost in the cotton gin suits. When he heard that the Congress was "about making some appropriations for procuring Arms etc. for the U.S.," here was a heaven-sent opportunity.[6] This would at least keep his manufactory going until he could get his cotton gin rights. The opportunity was so great and Whitney's situation so desperate that he was willing to promise "ten or Fifteen Thousand stand of Arms," a fantastic proposal! Whitney even promised to begin delivering "in a short time" and he "will come forward to Philadelphia immediately. . . ." New hope for a desperate man!

Why was such a rash proposal not rejected at once by such prudent men as President Adams and Timothy Pickering, the Secretary of War? The failure of the Pinckney mission had caused public concern, and French privateers were rumored to be off the coast. Even Washington was called out of retirement to head the armed forces. On the 4th of May 1798 Congress voted $800,000 for the purchase of cannon and small arms. When on the 24th of May Whitney arrived at the seat of government the plum was not only ripe and juicy but begging to be picked. Public sentiment was aroused, and the highest officials must do something—and that right promptly. Both sides could not close the contract quickly enough. Only the Purveyor of Public Supplies, Tench Coxe, seems to have kept his head—"I have my doubts about this matter and suspect that Mr. Whitney cannot perform as to time."[7]

It is not necessary to see "influence" at work here, though it is true that Whitney did have a number of Yale graduates who could help him. Much less is there any evidence that the generous terms of Whitney's contract grew out of the feeling that he had been shabbily used in his cotton gin suits. But he clearly had a personal friendship with Jefferson arising out of the patent for the cotton gin.

Certain features of the contract deserve closer examination. The legend makes much of the fact that the actual document was wholly handwritten. It says that all the other contractors of this time received printed contracts, and that there was therefore something special about Whitney's contract. Unfortunately an examination of the actual contracts, including Whitney's, in the National Archives shows that this was by no means the only handwritten contract–there were others, such as that of Owen Evans of Providence, Penn. The fact is that several of the early contracts were handwritten; the later contracts, mostly signed in September, were printed forms. These other contracts, printed or handwritten, were all identical in wording and provisions with Whitney's, except in the terms of the last paragraph. There *was* something special about Whitney's contract–it contained a paragraph six not included in any of the others. It was this paragraph that was crucial for Whitney. Having quickly sized up the situation in which the high officials found themselves, the shrewd Whitney saw his chance, consulted Baldwin as to the form the contract should take,[8] and at one stroke solved all his immediate problems. This paragraph reads:

> "6th. Five thousand dollars shall be advanced to the party of the second part on closing this contract, and on producing satisfactory evidence to the party of the first, that the said advance has been expended in making preparatory arrangements for the manufacture of arms, Five Thousand dollars more shall be advanced. No further advances shall be demanded until One thousand stands of Arms are ready for delivery, at which time the further sum of Five thousand dollars, shall be advanced. After the delivery of One thousand stands of arms, and the payment of the third advance as aforesaid, further advances shall be made at the discretion of the Secretary of the Treasury in proportion to the progress made in executing this contract. It is however understood and agreed by and between the parties to this instrument, that from time to time, whenever the party of the second part shall have the second thousand ready for delivery he shall be intitled to full payment for the same, so with respect to each and every Thousand until he shall have delivered the said Ten thousand stands."

Here was credit at last! Here was financial standing which assured further credit! Five thousand dollars at once, and five thousand more on terms which could be easily fulfilled by using his cotton

gin laborers and machines. And assured payment for each thousand stands of arms upon delivery—to a total of $134,000. Little wonder that Whitney wrote to his friend Stebbins: "Bankruptcy and ruin were constantly staring me in the face and disappointment trip'd me up every step I attempted to take, I was miserable . . . Loaded with a Debt of 3 or 4000 Dollars, without resources, and without any business that would ever furnish me a support, I knew not which way to turn . . . By this contract I obtained some thousands of Dollars in advance which saved me from ruin."[9]

No wonder that in his eagerness to read paragraph six of the contract, Whitney evidently skimmed rapidly over the incredible terms of paragraph one.[10] Whitney had contracted to deliver 4000 stands of arms by September 30, 1799, and 6000 more by September 30, 1800. Four thousand stands of arms in 15 months, from a factory yet to be built, and made by laborers as yet untrained, and by methods as yet unknown! And 6000 more in the following year! In his desperation Whitney had thrown all caution to the winds. He was no experienced manufacturer, as his deliveries of the relatively simple cotton gin indicate. He was aware that he knew nothing of arms making. And a prudent man would have expected at least some of the setbacks with which he fills his later letters to Wolcott, together with requests for further credit, contrary to the provisions of the contract. In short, despite his vague claims of new methods and what could be done by "Machinery moved by water," Whitney had only the vaguest idea of how he would actually fulfill the contract. He was not able to deliver even the first 500 muskets until September 26, 1801, and the contract was not actually completed until January 23, 1809. Further, the records of the Springfield Armory, now in the National Archives, show that even during the period 1815 to 1825, when his plant was fully established, Whitney never delivered muskets at the rate promised in his contract of 1798.[11]

Not only have these facts been forgotten in estimating Whitney's motives in the contract, but also in attempting a proper evaluation of his troubles with Samuel Dexter, who had replaced Wolcott as Secretary of the Treasury. We are asked to see Dexter as a villain abusing our hero with "malice" by demanding that he perform in accordance with his contract. The other contractors of 1798 had in many cases failed to fulfill their contracts, and some

of them had even gone into bankruptcy as a result of their efforts to manufacture arms for the federal government. But Springfield Armory records show that some of them had performed as contracted, a few on time and even more eventually. Yet these men had all ventured into arms making by financing themselves privately. There are no records of their writing the long apologetic letters full of troubles, promises, and requests for further advances, which characterize Whitney's correspondence from 1798 on. Nor had they been given the numerous informal extensions of time with which Wolcott, strongly under the influence of Hamilton's theories of the importance of manufactures, had favored Whitney and which culminated in a formal modification of the contract just before Wolcott left office. Whitney had been given more consideration than any other contractor.

But one might give at least a moment to the position of Dexter. He was a government official sworn to carry out the law and to protect the interests of the government. Whitney had been given every chance and had not performed. Some of the other contractors had. Even had Dexter seen the ultimate interests of the government in this matter in the broad terms that Wolcott and Jefferson did, he had no authority to make the extremely loose interpretation of Whitney's contract that Wolcott had. Actually Wolcott had left office partially as a result of other similar easy exercise of the discretions permitted his high office. Dexter did not deserve such blame.

And had Whitney, for his part, acted in good faith since 1801? We can leave out of our discussion the troubles Whitney so fully related in his numerous lengthy letters to Wolcott. They were real enough, even if recounted in rather unmanly fashion, but they were all of the sort, magnitude, and frequency which a prudent man would expect in an undertaking of this sort. And one could argue that if Whitney had been carelessly optimistic in what he had promised in the contract of 1798, so had the responsible government officials, who had also been warned by the Purveyor of Public Supplies to expect delay in delivery. However, despite Whitney's claims of the exhausting efforts and attention he had devoted to his arms manufacturing, the facts prove otherwise.

It is true that the lull in the affairs of Miller and Whitney from 1798 to 1801, plus the credit advanced him by the federal govern-

ment, did enable Whitney to devote most of his time in these years to make a beginning on fulfilling his arms contract, and by September of 1801 he did deliver the first 500 muskets. But from this initial delivery until 1807 there is no twelve-month period during which he delivered over 1000 muskets. During this same period he had been given advances from the Treasury such that he was constantly in debt to the United States. In fact, when Whitney finally completed delivery in January 1809 he received a payment of only $2450 as final settlement of the total contract of $134,000. Only on this date was his account up to date. The Whitney account in the Springfield Armory records[12] also shows that in 1806 Whitney delivered 1500 muskets, in 1807 he delivered 2000, and in 1808 and the first few days of 1809 he delivered 1500. What is the explanation of these facts?

I do not wish to imply that Whitney was misrepresenting his troubles in his letters to Wolcott, Dexter, and Dearborn; but he most certainly was not telling the whole story, as his other correspondence clearly shows. In April 1800 the Congress revised the patent law which had been the legal means of defeating Whitney's claims to his cotton gin rights. Under the new law Miller at once started suit against the principal offenders. The "lull" was over. But it became increasingly evident that justice would not be done Miller and Whitney in Georgia under any law. On September 4, 1801, Miller wrote to Whitney of a new possibility—their patent rights were to be purchased by the state legislatures. Here was a greater reward than Whitney could have dreamed of! Miller needs Whitney's help and his "contacts." Whitney can not wait and by November 22, 1801 is dating a letter to Stebbins: "Virginia Nineteen Miles North of the Northern line of North Carolina." He sees a chance that Miller and Whitney may get $100,000 from South Carolina alone for his rights—here was freedom from debt, assured financial security, and a credit reputation of the best. Better still, he will have the fame and prestige of a name officially cleared and full credit for his invention. Is it any wonder that a man of Whitney's ambitions and self-interest rushed off to Columbia and left the troubles and problems of arms manufacturing behind? Whitney had slaved and scrimped to get through Yale that he might become respected and financially secure. Now fortune beckoned, and the arms contract could wait.

From the fall of 1801 until Judge Johnson's decision of December 19, 1806 the ups and downs of cotton gin affairs were certainly far more important in Whitney's mind that the manufacture of arms.[13] This can be definitely established simply by noting the places where Whitney's and others' letters show him to have been in this five-year period. The contents of his correspondence clearly establish a similar conclusion, as does the mere volume of the lawsuits in which Miller and Whitney were engaged. In the final settlement of the partnership in 1818 Whitney was allowed $11,000 for the expenses of six journeys South on these lawsuits. Certainly he was seldom attending full time to the arms manufactory at New Haven.

In short, from 1801 to 1806 Whitney not only failed to fulfill the contract, he regularly substituted long letters of excuse for honest effort to carry out his obligation, while he chased the richer prize of the rewards he expected from the cotton gin. In the light of these facts Dexter can hardly be blamed for his actions, and Jefferson's intervention seems hardly to have been in the interests of the government, whatever effect it may have had upon the future of American industry.

II

Manufacture by the Uniformity Principle

The shortage of skilled artisans in the formative years of the American republic has been so often repeated as the source of Yankee mechanical ingenuity that it is now taken as axiomatic, without careful examination of the actual numbers as adequate for the needs of the day. This same axiom has served to "explain" Whitney's use of manufacture by interchangeable parts. In fact, Whitney so explains it himself. But let us look at the facts. The Springfield Armory was opened in 1794, and its payroll records from the beginning are to be found in the National Archives. By 1802 the Armory had 76 skilled armorers employed, and by 1814 it had 225. Although the figures for Harper's Ferry have not been preserved and we know it to have been substantially smaller than Springfield, we would be safe in assuming that Harper's Ferry had at least half this number of armorers. This total is impressive and seems hardly to indicate a scarcity of skilled armorers. In addition,

we have the records of deliveries by other private contractors of arms to the Springfield Armory. During the whole period which concerns us, either the Springfield Armory, or Asa Waters of Sutton, Mass., or Lemuel Pomeroy of Pittsfield, Mass., delivered at least as many arms in each year as did Whitney. In fact Springfield manufactured 16,120 in the six years from 1795-1801, a much more impressive record than Whitney's 10,000 in ten and a half years. Both started from nothing. Leaving out of account the deliveries of the smaller manufacturers, Springfield, Waters, and Pomeroy certainly had an ample supply of armorers—or are we to believe that they too had the principle of interchangeable parts which Whitney claimed was unique in his establishment at New Haven?

But where did Whitney get his ideas for manufacture of arms on this new principle? He always claimed that it was his and his alone, and so the legend says, despite strong evidence to the contrary. There can be no doubt that prior to Whitney other men had actually used the principle of manufacture by interchangeable parts. In the 1720's Christopher Polhem, in Sweden, was manufacturing gears for clocks by using machinery and precision measurement to ensure interchangeability.[14] But there is no evidence that Whitney or anyone in the United States knew of Polhem's work, though it could have influenced Blanc in Europe.

The work of Blanc [sic] was clearly known to Thomas Jefferson; in fact our legend always includes a recital of his letter to John Jay in 1785 describing Blanc's work, and Jefferson's letter to Monroe of November 14, 1801, in which he points out that by 1801 Whitney had not developed the method as far as Blanc had in 1788. But the most amazing thing about the Whitney legend is the failure of scholars to follow up this clear lead to answer two questions of first importance: (1) Who was Blanc and what did he do? (2) Did Blanc's work have any influence upon Whitney?

The sources on Blanc are not only easily available, but are very detailed on his methods and results, for much of his work was done in French government arsenals and created controversies which were the subject of several official investigations and reports.[15] Even a cursory examination of these sources would indicate that Whitney was far from being the first to introduce the principle of interchangeable parts in the manufacture of small arms. It is also quite clear that Blanc had carried the technique much further

than we have any evidence for Whitney's doing. Furthermore, Blanc's *Mémoire* of 1790 shows a profound understanding of the nature and probable effects of interchangeable manufacture, of which Whitney had only the barest inkling. Whitney's goal was only a system to use unskilled labor to increase output and reduce cost; whatever interchangeability he achieved was only a by-product of his method.

Blanc had problems to meet that Whitney never did. An entrenched officialdom and a threatened craft labor in long established government arsenals, together with the eclipse of the nascent industrial revolution in France under the Revolution and Napoleon, prevented a final fruition and spread of Blanc's ideas and methods in France and on the Continent.

But did the spark fly from Blanc to Whitney? A careful search in the correspondence of Whitney, Jefferson, Monroe, Jay, and Stiles indicates only that there were at least several paths by which it may very well have passed, of which the most likely is through Whitney's numerous conversations with Jefferson. But the only positive evidence seems to be a letter from Wolcott to Whitney dated 9 October 1798 in which he encloses ". . . a pamphlet on manufacture of arms . . . inform me freely and candidly whether the performance appears to you calculated to afford instructions to the workmen in this country. . . ." Whitney replied on 17 October 1798 that it was "misleading." Can this have been a copy of the report by le Roy on Blanc's *Mémoire* of 1790? We know that Jefferson was a regular subscriber to French publications, including the *Encyclopédie* as issued in parts. Did Jefferson's interest in Blanc in 1785 lead to receipt of this publication of the Académie des Sciences? And did he pass it on to Wolcott? One more bit of evidence remains. Writing to Wolcott on December 24, 1800, Whitney regrets that Wolcott does not have the leisure to examine "my whole plan and manner of executing the different branches of the work . . . to . . . compare them with the modes practiced in this and other Countries." Was Whitney actually familiar with the methods in use abroad, and if so, through what means? One must also admit that the language of many of Whitney's letters describing the merits of his methods are strangely reminiscent of Blanc's words. Yet we have nothing conclusive.

We must also ask whether Whitney's contemporaries in America

may have influenced him, in particular the work being done at the Springfield Armory. It is most significant that after signing the contract in June 1798 Whitney had gone to Springfield to see their methods and to talk with the superintendent.[16] And we have Whitney's letter in the summer of 1799 in which he had originally written "I might bribe workmen from Springfield to come to make me such tools as they have there." It is clear that Whitney was prepared to copy at least some of the methods already in use at Springfield. What were these machines? Unfortunately a fire in 1801 destroyed many of the records of the Springfield Armory, and the question cannot be answered fully. But we do have one official report[17] that gives clear indications that special machinery was in use by, at the latest, 1799: ". . . the artificers were employed for some time on the buildings, instead of on the manufactory, and in making the necessary pieces of machinery and tools. . . ." [If we take into account the difficulties of opening a new establishment, such as] " unsuccessful attempts in the proper construction of the machinery," [we should be satisfied with the present cost of muskets]. The report also uses such expressions as " The works now being complete, and labor-saving machines operating to great advantage . . . ," and ". . . improvements in the machinery and system for carrying on the manufactory."

That these improvements in the machines were effective is shown by the fact that in the month of September 1798 the Armory produced 80 muskets, but the following September 1799 it produced 442 muskets. This was accomplished with the same number of workers on the payroll. The report goes on to state that it had previously required 21 man-days to produce a musket; with the improved machinery only 9 man-days were needed. This at a time when Whitney had not yet delivered a single musket!

The later correspondence between Whitney and Roswell Lee, then superintendent at Springfield, although lacking technical details, strongly suggests that, contrary to Whitney's claims, at least a simultaneous development was going on. And there are patents, contracts, and accounts of Simeon North that strongly suggest that he, too, was using interchangeability in making his pistols as early as 1807.[18]

John Hall begun work on his rifle designed to be made by interchangeable parts and on machinery to manufacture it prior to his

patent of 1811 and was installing his methods and machines in the Harper's Ferry Armory by 1817. In 1827 Hall petitioned the government to give him adequate recompense for his contributions. This resulted in a series of commissions and investigations to establish the facts, by which he was finally compensated in 1840. The reports of these boards are matters of public record. The most significant for our purposes is one of 1827—two years after Whitneys' death.

> "In making this examination our attention was directed, in the first place, for several days, to viewing the operations of the numerous machines which were exhibited to us by the inventor, John H. Hall. Captain Hall has formed and adopted a system of manufacture of small arms, *entirely novel*, and which, no doubt, may be attended with the most beneficial results to the country, especially if carried into effect on a large scale.
>
> "His machines for this purpose . . . are used for cutting iron and steel, and for excuting woodwork . . . and *differ materially from any other machines we have ever seen in any other establishment . . . By no other process known to us (and we have seen most, if not all, that are in use in the United States) could arms be made so exactly alike as to interchange . . .*"[19] [Italics mine.]

This report was signed by James Carrington and Luther Sage, who had been government arms inspectors for years and were thoroughly familiar with the methods in use at Sprinfield and in the manufactories of the private contractors, including Whitney's. That Whitney himself thought Hall's work new is shown by the fact that he made the long trip to Harper's Ferry to see the "new system being adapted there."

A later report indicates that the machinery was especially desirable for it could manufacture "all other species of arms *identically*." This later report also shows that the machinery had been in use since at least as early as 1819 at Harper's Ferry: "At Harper's Ferry, and at Springfield, this machinery is believed to be *exclusively* used; and the money expended upon it, and upon the tools at the former armory from 1819 to 1834, both inclusive, was within a fraction of $150,000." The commission stated that since Hall was employed as an armorer at Harper's Ferry after 1819 he deserved no compensation in addition to his regular pay for improvements made after that date. But it recommended that he be compensated for the work he did from 1811 to 1819.[20]

All this can hardly be said to justify our legend's categorical statement "In every way Hall profitted by Whitney's work."

III

Manufacture by Machinery

We have thus far taken the term "manufacture by interchangeable parts" to have a clear meaning, based upon Blanc's, Whitney's, and Hall's dramatic demonstrations in assembling arms out of parts taken at random. This is a concept based upon characteristics of the product. It of course raises the question of how closely the parts must fit to be interchangeable. The usual answer is that the tolerances allowed must be sufficiently small for the product to work as designed and no more, since closer tolerances will merely increase cost. But this is rather vague. A more significant concept of interchangeable parts results from an examination of the actual methods by which such parts are produced. In this sense modern interchangeable parts require these elements: (1) precision machine tools, (2) precision gauges or other instruments of measurement, (3) uniformly accepted measurement standards, and (4) certain techniques of mechanical drawing. We do not, of course, expect Whitney to have all these elements, but we can estimate the contribution he may have made by comparing his work to them.[21]

In what sense were the Whitney firearms interchangeable? A test of a number of known Whitney arms in at least one collection proved that they were *not* interchangeable in all their parts! In fact, in some respects they are not even approximately interchangeable![22] The answer to this paradox is to be found partly in the actual means of establishment of standards for their manufacture. Each of the contractors of 1798 (and the later contractors as well) was given two or three samples of the Charleville model of 1763, and his contract specified that these were to be followed exactly. This method meant that at best the output of one plant would be interchangeable, but the muskets of a given contractor would not necessarily be interchangeable with those of the other contractors. In short, our third and fourth elements of interchangeable parts—uniform standards of measurement, and working from adequately dimensioned drawings—were absent. In fact, they were not to appear for two more generations.

However, the first steps in this direction were to be taken by John Hall. Writing to Congress February 21, 1840, he says: "And so in manufacturing a limb of a gun so as to conform to a model, by shifting the points, as convenience requires, from which the work is *gauged* and executed; the slight variations are added to each other in the progress of the work, so as to prevent uniformity. The course which I adopted to avoid this difficulty was, *to perform and gauge every operation on a limb from one point*, called a bearing, so that the variation in any operation could only be the single one from that points." [23] [Italics mine.]

What about our second element—use of gauges? Polhem had used these, and so had Blanc. There is clear evidence that gauges were being used at the Springfield Armory by 1801. Hall certainly had used them extensively before he went to Harper's Ferry in 1817, but there is not the slightest evidence that Whitney ever did.

A number of visitors went through the Whitneyville plant in Whitney's lifetime. All were properly amazed, but none wrote an account which tells us what Whitney's actual methods were, except that there were "moulds" and "machines." By putting bits of information together, the "moulds" can be interpreted as what would today be called die forging; Blanc had clearly used this method. But "moulds" may also refer to filing jigs. The legend makes much of: (1) the numerous references by Whitney and others to his "machines," (2) the machine tools listed in the inventory made by Baldwin of Whitney's estate, and (3) Whitney's supposed invention of the milling machine.

Let us examine each of these in detail. First, we may ask what did the term "machine" mean in Whitney's day? It most certainly did not mean what it does today. It included a trip hammer and a water wheel, but it also meant almost any kind of device. What machines did Whitney actually employ? In this connection we have the letter of ten-year-old Philos Blake, Whitney's nephew, written after his visit in September 1801: "Thare is a drilling machine and a boureing machine to bour berels and a screw machine and too great large buildings, one nother shop and a stocking shop to stocking guns in, a blacksmith shop and a trip hammer shop and five hundred guns done." This is the only first-hand evidence we have of Whitney's machines at this time. Yet an official inspection of the Springfield Armory in January 1801 says

the following: ". . . the number of Files required at the Factory being so great, some Water Machinery is now preparing which will diminish the demand of this expensive article."[24] Even more advanced machinery was used in the national armories by 1817.[25]

Timothy Dwight, one of Whitney's visitors prior to 1823, says: "Machinery moved by water . . . is employed for hammering, cutting, turning, perforating, grinding, polishing, etc."[26] But by this time we have clear evidence that such machinery was in use at both Springfield and Harper's Ferry.

The list of machine tools in the inventory of Whitney's estate is detailed and tells us much about the tools he had in use at the time of his death in 1825, but lists nothing not already in use at Springfield and Harper's Ferry. In fact, the large number of files listed as on hand would suggest that for much of his work Whitney used only a filing jig or fixture to guide a hand operated file as his principal means of producing uniform parts for the locks of his muskets. But Polhem had done this two generations earlier.

The Whitney papers at Yale also include a number of drawings, none of them dated, signed, or even identifiable as definitely made by Whitney; these drawings are quite possibly those of Whitney's nephews, for Benjamin Silliman, writing in 1832, says: "The manufactory has advanced, in these respects [machinery], since it has been superintended by Mr. Whitney's nephews, the Messrs. Blake, and to them it is indebted for some valuable improvements."[27] They had been in charge for about five years before Whitney's death.

The legend includes one specific machine—the milling machine discovered in 1912 by Professor Joseph W. Roe of Yale, and now in the collection of the New Haven Colony Historical Society. It was identified by Eli Whitney's grandson of the same name as having been made by his grandfather and as the first one ever made. His authority for this identification was that he remembered having seen it as a boy and having been told this story by workmen in the old Whitneyville plant. Roe dated this machine as of 1818 merely because of a statement in the *Encyclopaedia Britannica* that "the first milling machine was made in a gun manufactory in 1818." All this hardly seems adequate evidence. The first reference we have to the use of milling by Whitney is in his letter to Calhoun of March 20, 1823. But by 1818 we have clear evidence that

milling was in common use in both national armories, and by at least Robert Johnson and Lemuel Pomeroy of the private contractors.[28]

In short, we really know practically nothing of what Whitney actually had in his manufactory at Mill Rock; what little we do know of was clearly not an innovation; and we have good evidence to show that all that Whitney claimed as his own contribution was at least independently innovated by others, particularly in the national armories. Whitney's claims of originality seem to have been the exact opposite of the truth. Certainly no one is justified in stating that Whitneyville was the site of "the birth of the machine tool industry," much less the birthplace, even in America, of manufacture by means of interchangeable parts.

IV

"The Birth of American Technology"

There can be no doubt that what became by the 1850's widely known abroad as the "American system of manufacturing" had its origin in this first quarter of the nineteenth century and that its principal features were developed in the northeastern section of the United States. The American system included mass manufacture, by power-driven machinery, by machinery especially designed to serve its particular purpose, and by the use of the principle of interchangeable parts.

The legend says that all this stems from Eli Whitney. We have seen enough to indicate that we actually know very little of what he really did; hence there is no clear beginning from which we can tell what later developed from Whitney's work. It is also clear that other men were working along these very lines in the manufacture of arms at the same time.

The legend also claims that from Whitney stemmed the application of the American system of manufacture to many light metal-working industries–Colt and his revolver, Jerome's clocks, Waltham watches, Yale's locks, Singer's sewing machines, and so on. Even if we knew exactly what Whitney did, there is little evidence to support this application of the-great-man-in-history hypothesis. About all that can be said is that further applications of interchangeable parts would *logically* seem to follow from Whitney's broad *claims*. But this is not the same as proof that Whitney

actually had methods similar to those of later innovators, much less that they really did derive their ideas and methods from his. Certainly many other men contributed as much or more than Whitney, and evidence for their work can be found, far more convincing than Whitney's boasting claims. The legend says, for example, that the influence of Whitney was the basis of the Colt Armory methods of manufacture. In fact, it was E. K. Root who was the technical genius behind the manufacture of the Colt revolver, and his work stems directly from that of John Hall at Harper's Ferry. Whitney's influence on the manufacture of clocks, watches, and sewing machines is equally open to question.

We know so little of Whitney's actual methods of manufacture that his contribution to interchangeable parts is difficult to assess. What little we do know indicates, if anything, that Whitney was on the wrong track anyhow. John Hall's methods can be fairly clearly established, at least sufficiently for us to be sure that modern interchangeable manufacture derives far more from his inventive genius at Harper's Ferry than from Eli Whitney's manufactory at Mill Rock.[29] Actually one is led to find the origins of the "American system of manufacturing" in the culmination of a number of economic, social, and technical forces[39] brought to bear on manufacture by several men of genius, of whom Whitney can only be said to have been *perhaps* one.

V

Conclusion

This analysis of the Legend of Eli Whitney and Interchangeable Parts raises more questions than it answers. We have by no means arrived at the truth about the legend, much less about the advent of manufacture by interchangeable parts. However, I hope it is clear that the whole question needs re-examination—a more critical analysis of presuppositions and of the evidence which is known, and a more careful search for other sources.

But why not let this nice convenient legend go on? Were it Whitney alone that concerns us, that might be well enough. But the issue is larger than that. The history of our industrial growth is of first importance to the understanding of our American heritage. That industrial development cannot be properly understood

without careful consideration of its technological basis. Therefore the true story of the "Birth of American Technology" is of prime concern to us. We should make certain that the baby is perfect and legitimate.

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[1] J. Mirsky and A. Nevins, *The World of Eli Whitney* (N.Y., 1952); C. M. Green, *Eli Whitney and the Birth of American Technology* (Boston, 1956).

[2] B. Silliman in *American Journal of Science & Arts*, XXI (Jan. 1832), p. 256. "The private establishment of Mr. Whitney has proved a model for the more extensive manufactories which are the property of the nation. Into them, as the writer of the foregoing article has stated, and as I have been informed by Mr. Whitney, his principal improvements have been transplanted, chiefly by the aid of his workmen, and have now become common property." See also Whitney's numerous letters to government officials, e.g., EW to Wolcott, July 30, 1799; EW to Stebbins, April 26, 1800; Wadsworth to Wolcott, Dec. 24, 1800; Goodrich to Baldwin, Jan. 8, 1801; EW to Stebbins, Oct. 15, 1803; EW memo to War Dept., June 29, 1812; EW to Irvine, Nov. 25, 1813; EW to Calhoun, July 9, 1821; EW to Lee, Aug. 2, 1824.

[3] D. Olmstead, *Memoir of Eli Whitney, Esq.* (New Haven, 1846). First published in *Am. J. of Sci. & Arts*, XXI (Jan. 1832).

[4] New Haven Colony Historical Society Papers, Vol. V, p. 123.

[5] Miller to Whitney, Dec. 2, 1796 and May 11, 1797.

[6] Whitney to Wolcott, May 1, 1798.

[7] *The Report and Estimate of Tench Coxe, Purveyor of Public Supplies*, June 7, 1798.

[8] Whitney to Baldwin, May 27, 1798.

[9] Whitney to Stebbins, Nov. 27, 1798.

[10] Whitney admitted as much in his letter to Wolcott of Oct. 17, 1798, and again June 29, 1799. He had already admitted in a letter to Stebbins: "I have now taken a serious task upon myself and I fear a greater one than is in the power of any man to perform in the given time—but it is too late to go back."

[11] Note that when Whitney had his plant fully established and his contract of 1798 was about to run out, he wrote to Major Rogers (Oct. 28, 1809) looking for a new contract: "I would contract to deliver 2000 or 2500 pr year for seven or ten years." Experience had sobered his promises.

[12] Springfield Armory Records, National Archives.

[13] EW to Stebbins, Oct. 15, 1803.

[14] C. Polhem, *Patriotiska testamente* (1761).

C. H. König, *Inledning til mechaniken och bygningskonsten jämte en beskrifning öfwer atskillige af framledne . . . Hr Polhem opfundne machiner* (1752).

Christopher Polhem Mimesskrift Utgifren af Svenska Teknologföreningen (Stockholm, 1911), pp. 121-169. See also the machines, models, and drawings of Polhem in the collections of the Tekniska Museet, Stockholm.

(The author is preparing a summary of the principal features of Polhem's work in this and other important advances in mass production.)

[18] H. Blanc, *Mémoire Important sur la fabrication des armes de guerre, à l'Assembleé Nationale* (Paris, 1790).

Le Roy, *et al., Rapport fait à l'Académie Royale des Sciences, d'un Mémoire Important de M. Blanc* (Paris, 1791).

Comité Central de l'Artillerie, Archives, 6-c-5: 6th Division, 1st Chap. File 6202. (These records are located at the Laboratoires Central et Ecoles de l'Armament, Fort de Montrouge, Arcueil [Seine] and are quite extensive and complete.)

For a convenient summary see W. J. Durfee, "The First Systematic Attempt at Interchangeability in Firearms," *Cassier's Magazine*, V (Nov. 1893–April 1894), pp. 469-77.

For the later history of Blanc's methods see:

H. Cotty, *Mémoire sur la Fabrication des Armes Portatives de Guerre* (Paris, 1806). (Copies are at the U.S. Military Academy, West Point, N.Y., and the Bibliothèque Nationale.)

and M. Bottet, *La Manufacture d'Armes de Versailles, Boutet Directeur-Artiste* (Paris, 1903).

(Working independently Prof. John E. Sawyer of Yale University had by 1959 already completed a study of Blanc. His project is now being broadened and is expected to result in publication and translation of the significant sources and a detailed analysis of the Blanc record.)

[16] Whitney to Wolcott, June 29, 1799.

[17] *Executive Documents, 6th Congress, 1st Session*, January 7, 1800, "Springfield Armory," passim.

[18] See J. E. Hicks, *United States Ordnance* (Mount Vernon, N.Y., 1940), Vol. II, Chap. VII, where much of the material is reprinted.

[19] *Reports of Committees, No. 375, 24th Congress, 1st Session* (1836), John Hall Petition, Document Number 4, "Extracts from Report of Board of Commissioners to the Colonel of Ordnance in 1827 to examine the machinery," pp. 8-9.

[20] *House Reports No. 453, 26th Congress, 1st Session* (1840). John H. Hall Petition, U.S. Document 671.

[21] For a more complete analysis of the concept of interchangeable parts and for a detailed history of its development see the author's forthcoming monograph *History of Shop Precision of Measurement and Interchangeable Parts.*

[22] C. E. Fuller, *The Whitney Firearms* (1946), p. 156.

[23] *House Reports, No. 453, 26th Congress, 1st Session* (1840), John H. Hall Petition, U.S. Document 671, p. 6.

[24] Lt. Col. John Whiting, *Report on Inspection of Springfield Armories*, to Hon. William Eustis, Esq., Secretary of War, dated Boston, January 13, 1801. Report Number Two.

[25] *American State Papers, Military Affairs, 17th Congress*, No. 246, "Armory at Springfield, 1795-1817."

[26] T. Dwight, *Travels in New England and New York* (Edinburgh, 1823).

[27] Silliman, *loc. cit.*

[28] For further details see R. S. Woodbury, *History of the Milling Machine* (Cambridge, Mass., 1960).

[29] For this story see the author's forthcoming monograph *History of Shop Precision of Measurement and Interchangeable Parts.*

[30] See J. E. Sawyer, "The Social Basis of the American System of Manufacturing," *Journal of Economic History* (1954), especially pp. 369-79.

Book Reviews

Adventures with the Missing Link. By Raymond A. Dart, with Dennis Craig. New York: Harper and Brothers, 1959. Pp. xxiv, 255. Index. 62 illustrations (figures, plates, maps, and charts). $5.00.

The South African discoveries of early man, or apes, and early cultures have come with increasing frequency in the last decade. They have done much to alter our understanding of human and cultural evolution. From a figure central to this investigation comes a personal narrative and interpretation which may serve as an inspiration and warning to students in the field. As such it is not a new approach, for similar personal accounts have come from the pens of other major contributors to prehistory. The author outlines the long history from the first discovery of Australopithecus to its recent recognition as a crucial fossil form.

The investigation of Australopithecine remains is closely linked to efforts to define hominid status, and this, in turn, to questions of the origins of tools. It is partly due to these discoveries by Dr. Dart and his associates that the criteria of definition have changed in recent years. Not long ago hominid status tended to be accorded a fossil form on the basis of cranial capacity, without much cognizance taken of the wide range evidenced by "effective" individuals among contemporary men. The persistence of this viewpoint has led in some quarters to controversy over these fossil South Africans. Recently a counter tendency has developed, ignoring for the most part gross brain size and stressing instead the developmental status of the post-cranial skeleton. This view is closely focused on the adoption of an habitually erect posture with attendant freeing of the fore-limbs, greater sense of balance, and enhanced hand-eye coordination. Closely related to this, in fact dependent on it, is the development of tool-using and tool-making.

Traditionally among prehistorians man's first tools were of stone, whether deliberately shaped as were the early pebble tools or simply used as argued by proponents of an eolithic age. (Most prehistorians would, however, admit the possibility of even earlier tools of perishable materials, such as wood.) Dr. Dart takes issue with those who favor the primacy of lithic tools and urges recognition of an earlier osteodontokeratic (bone-tooth-horn) age. The Australopithecine sites contain large quantities of animal bone, the pieces numbering in the thousands; these are not indiscriminate collections. In fact the majority

of pieces are those most usable for tools, and many have been split, possibly by twisting, to make them more useful for this service. The selected nature of the bone remains, their particular form, and the habits of certain nominally bone-collecting animals (e. g., the hyena) are discussed at length. The author presents convincing evidence that these assemblages of bone are not fortuitous nor are they the work of wild animals. The osteodontokeratic tools, furthermore, are alleged to be more efficient than the generality of Lower Palaeolithic stone tools. The bone blades could well have had effective edges; the bone daggers were doubtless better piercing tools than the stone hand-ax; the jaws, complete with teeth, were certainly good slashing or splitting weapons. To support this argument further, the fractures of animal bones, primarily skulls, were examined; these were judged to be post-mortem breaks and the product of particular blows with certain of the bone tools. Some of this damage was duplicated experimentally, using fresh skulls and replicas of the tools.

In his investigations into the origins of these bony fractures and the dismemberment of skeletons, Dr. Dart enlisted the aid of a colleague specializing in forensic medicine. This assistance was not amiss, for it appears that the Australopithecines murdered their own kind and probably ate them. If indeed they were cannibalistic, this behavioral pattern, found in Europe and the Far East during the Pleistocene, is of great antiquity.

For those persons interested in the earliest beginnings of technology, the author provides lengthy discussions of basic matters: the differences between humans and the living apes in precision of manipulation; similar differences in the focus and time span of attention; the early appearance of dextrality; the relative merits of bone and stone tools; the relation between tools and dietary habits; and the necessity of weapons to protect an organism which had exposed its vulnerable ventral aspect by assuming an erect posture.

To only one of Dr. Dart's major theses would this reviewer take exception. Dart argues that verbal communication among early hominids was minimal, even as late as Neanderthal Man, whom he supposes to have used only the onomatopoetic sounds of the pre-speaking child. The basis for this position is the cultural poverty of these people and the thought that language would only have developed when there was something important to talk about. Regrettably the nature of cultural remains from this period is such that many probable cultural aspects are not represented. However, there are two circumstances which have some relevance to this matter. The central Australian aborigines are noteworthy, perhaps even infamous, among anthropologists for the complexity of their social organization and the body of supporting mythology; they are equally well known for their simple material culture. Their languages are well developed, not rudimentary. Secondly, the late Lower Palaeolithic and Middle Palaeo-

lithic remains of Europe give evidences, largely circumstantial, for the development of rudimentary religion; at least one finds deliberate burial of the dead, with grave goods, suggesting belief in an afterworld. This activity must have been accompanied by appropriate speech. All in all, most anthropoligists and linguistics hold that language is coterminous with man and would find it unthinkable that during the bulk of human existence, in point of time, communication was confined to cries and gestures.

This short volume is well produced with many illustrations, charts, and maps. Unfortunately the half-tones are very contrasty and dark, making the discernment of detail virtually impossible. One figure (35) has been given the wrong caption, a duplicate of that for figure 36. The selected bibliography lists seventy-one wide-ranging items, and the index is most thorough and evidently accurate.

A final note for members of this Society. Those who profit through this volume owe a double debt of gratitude to the Wilkie Foundation. Not only has this foundation underwritten some costs of the Society, but it has handsomely supported Dr. Dart's field researches and travels over a period of years.

R. F. G. Spier *

Africa: Its Peoples and Their Culture History. By George Peter Murdock. New York: McGraw-Hill, 1959. Pp. xiii, 456. Index of Tribal Names. 4 Tables. 17 Maps. 23 Photographs. $8.75.

The book starts with 47 pages of "Orientation": In the first three chapters, Murdock discusses the geography of Africa, its races and languages. The next three introduce the reader to the different types of economy in Africa and to the forms of social organization and government. The final chapter of the Orientation reviews older attempts to search into African history, presents Murdock's own "methods of reconstructing the past," and, lastly, gives a "synopsis of the major historical movements on the continent as a whole over the past 7,000 years."

Chapters 8-55 (pp. 48-421), the main body of the book, investigate the cultural provinces into which Murdock divides Africa. They summarize and order ethnographic knowledge concerning all known African tribes (which have so many synonymous names that the index lists almost 6,100). The mere attempt to classify them into some 850 groups, these into 48 culture provinces, and these in turn into ten sections, is truly worthy of the highest praise. So is the tribal map of

* Dr. Spier, Assistant Professor of Anthropology at the University of Missouri, has published papers on change and stability in the material culture of certain California Indians and of the immigrant Chinese in California.

Africa which shows the location of more than 830 tribes. The work should become indispensable as a reference book.

The claim (on the dust cover) that "this is the first book in the English language to present such a comprehensive survey of the peoples, culture and history of the African continent," although apparently made by the publisher, actually is an understatement. We should like to hail the book as nothing less than revolutionary: one of the leading American anthropologists, a winner of the Viking Fund medal in anthropology, a past president of the American Anthropological Association, has dedicated eight years of research to classification and historical reconstruction! May we hope that the turn of the tide has come?

However, as enthusiastically as we support the attempt of the book to revive historical research in anthropology, we cannot agree with a number of its methods. Murdock covers all of Africa geographically. Excellent. But while it is complete, geographically, this book on the culture history of Africa actually excludes the major part of culture. It restricts itself to the study of "food-producing activities, the division of labor by sex, housing and settlement patterns, kinship and marriage, the forms of social and political organization, and a few miscellanea such as cannibalism and genital mutilations" and omits "such important fields as religion, art, law, socialization and technology" (p. viii). We admire the author for what he has achieved. Admittedly, no man can do everything; but the omission of the fields mentioned makes the book, as a culture history of Africa, incomplete.

Excluding the arts and technology has resulted in an even more disastrous defect than mere incompleteness of ethnographic description: it has apparently led to neglecting indispensable sources. Although Murdock did use the descriptive literature, there is no indication that he made use of the many objects gathered in dusty and musty storage rooms and showcases of ethnographic museums, objects whose importance is not limited to the study of art and of technology. They constitute indispensable sources for the study of cultures in their entirety, and even for the study of the majority of those segments to which Murdock restricts his work. No reconstruction of the culture history of a continent is possible without complete "evidence from ethnographic distributions" which, as Murdock admits, "contributes to the solution of" many a problem (p. 179). An anthropologist who does not make use of the treasures available in the museums might be compared to a linguist refusing to make use of vocabularies. The culture history of Africa cannot be written without years of work in museums, and for that reason an extended stay in Europe may be many times more important than a year or two in Africa.

Murdock, however, in the very first paragraph of the book seems to apologize for his lack of field experience in Africa. We do hope that he did it with tongue in cheek. Even the most orthodox believer

in field work as the anthropologist's decisive qualification must realize that no one man can do respectable field work among several thousand tribes and gain personal knowledge of a continent of eleven to twelve million square miles; even if he had studied at first hand one or two tiny specks of such an enormous mass, his own field work, methodologically, must be considered as but one single source among the thousands of other sources. We are certain that Murdock, in this respect, shares our views. Frobenius, for example, devoted his life to field work in Africa, again and again returning there to acquaint himself with as many parts of it as he possibly could. Still Murdock, we assume, considers his work "definitely bad" since it is not mentioned at all in the book and thus clearly falls into the category which Murdock, accordingly, "simply ignores" (p. 40).

For the reconstruction of the culture history of Africa, Murdock combines "six independent methods": the use of contemporary written records (mainly for North African history), archaeological evidence, linguistic relationships, evidence from cultivated plant geography, inference from social organization, and "ethnographic distributional analysis"; the last, however, Murdock restricts to "areas of continuous distribution of cultural forms." By so doing Murdock raises the lesser "criterion of continuity" above the criteria of form and quantity which, even if applied alone, are decisive, and which Murdock himself calls "admirable" (p. 41). If a linguist were to fall into the same methodological error, he could not regard Roumanian as a Romance language, nor Tocharian as Indo-European, nor Hungarian as Finno-Ugric.

Still in certain instances, Murdock is prepared to bridge large distances by a hypothesis. In others he is not. Thus, he becomes almost emotional in condemning the attempts to search for distant connections of Hottentot culture ("sheer nonsense," "no reasonable alternative"), stating that with exception of the "pastoral mode of life," "all other aspects of their culture . . . show such striking affinities with that of the Bushmen" that they must be considered a "Bushman group who adopted cattle." A study excluding religion, art, technology, and a number of other aspects of culture seems hardly qualified to make a statement concerning "all . . . aspects of . . . culture." All? Even two traits of Hottentot culture which do belong to the "miscellanea" dealt with by Murdock are not mentioned, namely, circumcision and the excision of one testicle. Granted, the sources disagree on both points. But no critique of sources is offered; the sources which give detailed descriptions of the practices involved are not listed. Thus three problems are swept under the carpet: the reliability of the sources in question, the proper ethnographic description of Hottentot culture, and its relationship to cultures outside the immediate vicinity.

The publisher advertises the book as "an upper-division and first-year graduate textbook." It is far more. It contains an abundance of

ideas which deserve discussion. (However, they cannot easily be found since there is no general index, nor does the table of contents offer access to them.) At least a few should be quoted:

"Agriculture was independently developed . . . by the Negroes of West Africa. This was . . . a genuine invention, not a borrowing from another people." (p. 64)

The "trans-Pacific spread [of the sweet potato] must have taken place very much earlier than even the most uninhibited theorists have as yet dared to assume." (p. 224)

Concerning the East African hunters: "No other region of comparable size in the entire world . . . reveals so high a degree of cultural uniformity. . . . [This] singular cultural uniformity . . . must have an explanation. Among possible reasons the author proposes the influence of local exogamy. . . . Local exogamy . . . makes it possible for diffusion to operate through the socialization process . . . and . . . produce a high degree of cultural uniformity over wide areas." (pp. 62 f.)

"The constitution of Carthage . . . commonly compared . . . with those of ancient Greece [and] early Rome . . . can now [be understood] as an obvious borrowing from the political institutions of the Berbers." (p. 151)

These quotations, too few and too short, can neither convey the importance of the hypotheses nor indicate whether the reasoning behind them is sound. It must be deplored that a book presenting such challenging ideas follows in many respects the style of textbooks: no footnotes; only occasional references to other authorities; selected bibliographies at the end of separate chapters instead of complete lists of the works consulted. It would be unfortunate indeed if its textbook character and its occasional bow to the "general reader" (p. 24) were to prevent the specialist from scrutinizing the book hypothesis by hypothesis.

PAUL LESER *

The Science of Mechanics in the Middle Ages. By Marshall Clagett. Madison: University of Wisconsin Press (London: Oxford University Press), 1959. Pp. xxx, 711. Illustrations. $8.00.

The most important change of perspective in the history of science during the twentieth century was effected by the publication of Pierre Duhem's *Origines de la Statique* and *Études sur Leonard da Vinci* in the years 1906-13. Re-reading long-forgotten scholastic philosophers

* Professor of Anthropology at the Hartford Seminary Foundation, Dr. Leser has done field work in Africa and Europe and has published studies in agrarian anthropology and anthropological methodology.

of the later Middle Ages, Duhem found in their writings anticipations of concepts—including the principle of virtual displacements, the idea of static moment, the definition of uniform acceleration, and the law of inertia—on which the modern science of mechanics was founded during the seventeenth century. Far from consisting of sterile absurdities, these medieval texts, deriving from the works of Euclid, Archimedes, Aristotle, and other giants of antiquity, contained long passages devoted specifically and constructively to the very problems tackled by Galileo at the dawn of modern science. Nor was it difficult to show how the language and procedures of Galileo echoed those of his predecessors, though he went beyond them. The beginning of the criticism of Aristotle's physics thus had to be set back three hundred years before the seventeenth century. Hence, in that very branch of science which was outstanding for the rapidity and extent of its achievement in the early modern age, it seemed that the medieval was no period of negation; on the contrary, it was a period when the way was prepared for future substantial progress.

In the last fifty years great scholarship has been applied to the study of the materials only partially explored by Duhem and the conclusions he drew from them. Scores of manuscripts have been unearthed, and the course of mechanical theory has become clearer as a result. While Duhem's chronology and his detailed interpretations have been modified in many respects, his broad conclusion that the accomplishments of Galileo and his immediate successors in the science of mechanics were the consequences of extensions and refinements of medieval ideas has only been confirmed. There is indeed a sense—though a far more restricted sense than Duhem recognised—in which modern physical science began in the fourteenth century.

For over a decade Professor Clagett has been the leading American figure in this exacting branch of scholarship. His command of the sources in European libraries and his familiarity with the whole subject are unrivalled; furthermore, he has formed at the University of Wisconsin the most active center in the world for the study of medieval physics. And in this capacious volume he has presented, for the first time, a full review of theoretical mechanics from the Greeks to Galileo, with chief emphasis on the fourteenth century when so many new and profound ideas were discussed. This is no light task; as the author imposes the highest standards on himself, his book is not light reading. The bulk of it consists of passages from the original authors translated into English (Latin texts are also provided when the originals are inaccessible), but no less interesting and informative to the majority of readers will be the learned and balanced introductions which Professor Clagett has prefixed to each group of translations. Thus in effect *The Science of Mechanics in the Middle Ages* provides both a narrative history—though one that inevitably can only be summary in its analysis—and an ample selection of the original works now made

readily available for the first time. It is not easy to express one's sense of indebtedness to Professor Clagett for this fine achievement. The history of mechanical theory from the thirteenth century to Galileo having become (since Duhem) one of the most fascinating and controversial areas in the history of science, it has been an arduous enterprise for the non-medievalist to find his way among the thicket of specialist publications and unobtainable sources. Now his way has been made—I will not say exactly easy—but passable.

The book is divided into three main sections: *Medieval Statics* (Greek and Arab Forerunners, Medieval Latin); *Medieval Kinematics* (Origins, Merton College, the Theorem on Uniform Acceleration, Application of Geometry); and *Medieval Dynamics* (Aristotle and Bradwardine, Impetus Theory, Free Fall of Bodies, Mechanics and Cosmology). A final section traces the *Fate and Scope of Medieval Mechanics*: here the last chapter of all, "Medieval Mechanics in Retrospect," might I believe prove enlightening to some readers read *before* the main bulk of the book, since it provides an admirable and succinct statement of the twenty principal theoretical propositions that the medieval philosophers first formulated. It is then possible to go back and follow these individually, first in Professor Clagett's introductions and then in the text themselves.

This work provokes some reflections on the strange history of the relations between mechanical science and applied mechanics, or the design of machines. "We are told by Diogenes Laertius," writes Professor Clagett, "that Archytas was the first to expound mechanics by means of mathematical principles . . . it seems reasonable to suppose that Archytas treated the same basic instruments [the five 'simple machines'] into which later authors resolved all machines: the lever, the wheel and axle, the pulley, the wedge and the screw." So mechanics began with machines, all of them reducible in explanation of their action to the principle of the first, the lever, which was the root of the abstract science of statics. Down to *Le Mecchaniche* of Galileo (written in the 1590's, first published 1634), this analysis of the five simple machines was the sole contribution of mathematical science to engineering technology. On the other hand, there was an immense development of the theory of motion, stemming from two origins: (1) questions about the movements of the heavenly spheres; (2) the discussions of Aristotle in his *Physics*. This development split into two traditions (often found together in the same treatise), that of kinematics and that of dynamics. Far removed now from the infinite series, the geometrical diagrams, and the complex technical vocabulary of the schoolmen were the millwrights and others who discovered how to employ the crank, tappet and cam, the eccentric, the ratchet, gears, and other means to harness wind and water to the needs of clothmaking, mining, and metal-working; above all to construct that great marvel of medieval mechanical ingenuity, the clock. What con-

ceivable relevance could a learned dissertation *De configurationibus qualitatum* have for them? And how mistaken these practical men would have been, as we now know, to despise it. For analysis of the intensity of qualities led to the concept of uniform acceleration and its graphical representation, that in turn to the theoretical mechanics of the seventeenth century, from which again sprang the applied mechanics of the eighteenth century and the thermodynamics of the nineteenth. Ultimately the union of science and engineering, broken for two thousand years, was restored to bring mechanics and machinery together once more. Professor Clagett's book covers an important phase—however superficially unpromising at the time—of this story.

A. Rupert Hall *

European Armour, circa 1066 to circa 1700. By Claude Blair. London: B. T. Batsford, Ltd., 1958 (New York: The Macmillan Co., 1959). Pp. 248. Illustrations. $7.00.

This is the first general survey of the history of body armour to appear in English since the monumental five-volume work, *A Record of European Arms and Armour* by Guy Francis Laking, which appeared between 1920 and 1922.

Although Laking's volumes remain a major reference in English and are still valuable because of their hundreds of reproductions of specimens, like most first explorations, they contain a number of errors and reproduce as genuine specimens which we now know to be forgeries—composite or altered. Blair's new book, while by no means as bulky, covers much the same ground, incorporates what has been learned during the intervening years, and does not perpetuate the erroneous conclusions of earlier writers.

The author, formerly on the staff of the Tower of London and now Assistant Keeper in the Metal Work Department of the Victoria and Albert Museum, has devoted years to the research which is incorporated in the present study.

The result is a most valuable contribution to the study of European arms and armour, certainly the essential volume for any library or collector interested in this field or, indeed, in the history of costume. It can be considered now the basic reference work in English—for Laking is not only out of print but must be used with caution by any but an expert armed with the knowledge of its mistakes. Although Blair's work is directed primarily to the person with a general interest, it contains matter of value even to the advanced collector.

* Professor Hall, now at the University of California (Los Angeles), is author of *The Scientific Revolution: 1500-1800* and one of the co-editors of the five-volume *A History of Technology*.

The book is divided into eleven chapters, plus appendix, bibliography, references, index, list of illustrations. The chapter headings indicate its scope:

The Age of Mail, c. 1066-c. 1250
The Introduction of Plate Armour, c. 1250-c. 1330
Early Plate Armour, c. 1330-c. 1410
The Great Period, c. 1410-c. 1500
The Sixteenth Century, c. 1500-c. 1600
The Decline, c. 1600-c. 1700
Tournament Armour
The Decoration of Armour
The Shield
Horse Armour
The Making of Armour

It is a pity that the author was apparently restricted in the format of the book, for this volume would have had a handsomer appearance if it could have been produced in a larger page size and with a more liberal use of half tone reproductions. It also seems that the text has been condensed to the utmost, which affects its readability, requiring as it does very close concentration.

The work is copiously illustrated with reproductions of contemporary source material, sculpture, illuminations, embroidery, paintings, surviving specimens, and over 230 line drawings. In sum there are 300 illustrations, all of which are well selected to illustrate points brought out in the text. The quality of the reproductions is excellent, although they too would have been better if larger.

The chapter on the making of armour runs to less than three pages, of which approximately one page is devoted to armourer's marks. Obviously this leaves space for only an extremely condensed treatment of the technique of armour making.

This book is recommended for those interested in the history of the development of body armour in Europe. It does not treat of weapons.

Hermann Warner Williams, Jr.*

* Dr. Williams is Director of The Corcoran Gallery of Art, Washington, D. C.

Pioneers in Printing. By Sean Jennett. Fair Lawn, N. J.: Essential Books, 1959. Pp. xi, 196. Illustrations. $4.50.

U Istokov Russkogo Knigopechatanija (Sources of Russian Printing). Akademija Nauk SSSR, Moscow: Otdelenie Istoricheskix Nauk, 1959. Pp. 268. Illustrations. 15.10 rubles.

These two books on the history of printing differ greatly in approach, style, and subject-matter. The first, written in a popular style, uses a biographical approach to narrate the development of Western European printing; the second is a symposium reflecting the achievements of contemporary Soviet scholarship in the field of Russian and Slavic incunabula.

The book-jacket of Jennett's work asserts that "The aim of the book is not to recount the technicalities of machinery, but rather to present each character as a living man engrossed in his ideas and pursuing them persistently, in conflict or in agreement with his period." In this reviewer's opinion, the author succeeds only partially in this task. Through colorful sketches of significant figures (Johann Gutenberg, William Gaston, William Caslon, John Baskerville, Alois Senefelder, Frederick Koenig, Ottmar Mergenthaler, and Tolbert Lanston), Jennett tells the story of typographical development, but his sometimes inadequate account of the social and cultural background makes the book seem little more than a string of loosely connected biographies. Furthermore, certain printers, such as Anthony Koberger and Aldus Manutius, who made significant contributions to the development of typographical art, are omitted. Despite the omissions and imperfections, this smoothly written publication can be recommended for the layman interested in the historical development of printing.

In the second volume under consideration, the Historical Section of the Academy of Sciences of the USSR presents a collection of scholarly articles commemorating the 375th anniversary of the death of Ivan Fedorov—the "father" of Russian typography. Written by various Soviet scholars, each article deals with the early period of Russian printing. The volume also contains historical sources and a selective bibliography of works on early Russian printing.

In the first article, "The Origin of Printing in Russia," M. N. Tixomirov analyzes the beginning of Slavic typography relative to Fedorov's later printing activity in Russia. Founder of Cyrillic printing was Shvaipol Fiol, who, in 1491 in Cracow, published the first Slavic books designed for Ukrainians. The second Cyrillic press appeared in Cetinje, Montenegro, where, under the guidance of the monk Makarius, church books were printed; the next Cyrillic press appeared in Venice and, later, another in Prague. In 1564, Ivan Fedorov and his co-worker, Peter Mstislavets, printed the first dated Russian book, *Acts of the Apostles*, in Moscow. However, the author dates the origin of Russian

typography as the year 1553, on the basis of a newly discovered copy of the "Totemsky Chronicle," and he assumes that printing was regarded favorably in Russia in the sixteenth century. In order to defend this hypothesis, the author states that Fedorov did not flee from Moscow to the Grand Duchy of Lithuania and Ukraine in the year 1564-1565, but that he left his country of his own "free will," and on recommendation of Czar Ivan IV (p. 37). This is one of the greatest weaknesses of Tixomirov's hypothesis, for Fedorov himself says at the end of *Acts of Apostles* (Lviv, 1574) that he was "exiled" and "transported" because "many officials and heads of Church and teachers" charged him with "many heresies." He attributes these charges to his persecutors' "hate and envy" and their ill-nature and ignorance. (Metropolitan Ilarion, "Pervodrukovanyj Lvivs'kyj Apostol 1574 r.," *Naukovyj zbirnyk* [Ukrajins'ka Vilna Akademija Nauk, New York], II [1953], pp. 25-26.)

Most scholars agree that Fedorov and Mstislavets were forced to flee in 1564, after their printing house was burned by illiterate Russian clergy. Other information confirming this episode is provided by Giles Fletcher, who was sent by Queen Elizabeth as an ambassador to Moscow during 1588-1589, in his work, *Of the Russe Common Wealth*, ... Fletcher's book was published in London in 1591, and the Muscovite government in 1591-1592, through the English merchants who were at that time in Moscow, requested William Cecil, adviser to Elizabeth, to prohibit the sale of this book in London and to have the whole edition burned.

Like most Russian scholars, Tixomirov denies that Fedorov's printing establishment was destroyed, referring to Fletcher's statement as biased. In this particular case, the consensus of Russian scholars is not supported by the historical sources. On the contrary, the Muscovite clergy saw typography as competition to their own work of manuscription, and this was one of the main reasons for the destruction of Fedorov's press. The author also fails to mention the tragic fate of Bartholomew Gholton, the Lübeck printer who was invited to Russia as an interpreter for the Czar's Foreign Office, and who was drowned by the Muscovites. Because of these discrepancies, the picture of Fedorov's printing activity in Russia is distorted.

A. A. Sidorov in his paper, "The Artistic-Technical Characteristics of Early Slavic Incunabula," carefully analyzes European printing influences on Russian typography. His basic attention is directed toward Fiol's and Fedorov's printed books. Comparison of the different type-faces being used in Eastern Europe indicates Fiol's direct influence on Fedorov's printing later. Unfortunately, the bibliography of this article omits the recent and important monograph on Fiol by Karol Heintsch, *Ze studiów nad Szwaipoltem Fiolem.* I: *Materiały do zyciorysu i działalnosci Fiola* (Wrocław, 1957).

H. I. Koljada's article, "The History of Printing Relations among

Russia, Ukraine, and Roumania in the Sixteenth and Seventeenth Centuries," is an earnest attempt to trace the Slavic influence on the early stages of Roumanian (Moldavian) typography. Fedorov's *Acts of the Apostles* and the Ostrohian *Bible* (1581) served particularly as examples for early Moldavian printing.

E. V. Zatsepina, in her "Contribution to the Question of the Origin of Ancient Printed Ornamentation," sets forth an excellent description and analysis of early Russian manuscripts and their ornamentation. She notes the direct influence of Byzantine, Balkan, and Italian Renaissance art on Russian typography. This study is illustrated with significant Russian manuscript ornamentations, and the wealth of evidence is expertly managed without obscuring the outline of the article.

The second part of the volume, "Source Materials," contains "A Description of Russian Incunabula" by T. N. Protas'eva, and "Tales of the Origin of Muscovite Book Printing" by Protas'eva and M. V. Shchepkina. These articles include texts and translations from Church-Slavic into Russian of historical sources related to early Russian printing. The third article, "Translations of Prefaces and Epilogues of Incunabula" by Shchepkina, reproduces important Church-Slavic texts translated into Russian. In "Description . . . ," the authors include not only Russian incunabula, but also Byelorussian and Ukranian.

The last part of the volume, "Appendices," contains supplements and bibliography to the Russian incunabula.

One of the major defects of this volume are the errors in the terminology employed by the Soviet scholars, who tend to confuse the Ukraine of the sixteenth century and Byelorussia with Muscovy, the Russia of the Tsars, and who therefore wrongly include the incunabula printed in Zabludiv, Lviv, and Ostroh as part of the history of Russian printing. This "patriotic" interpretation of Russian cultural life in the sixteenth century therefore does not give an objective picture of the development of early Russian printing. Although the volume is notable for its many illustrations, its usefulness suffers from the lack of an index.

LUBOMYR WYNAR *

* Dr. Lubomyr Wynar, Instructor and Assistant Librarian at the Case Institute of Technology, is the author of many articles pertaining to the history of eastern Europe during the sixteenth and seventeenth centuries. He is a member of the Historical Section of the Ukrainian Academy of Arts and Sciences.

A Diderot Pictorial Encyclopedia of Trades and Industry: Manufacturing and the Technical Arts in Plates Selected from "L'Encyclopedie, ou Dictionnaire Raisonné des Sciences, des Arts et des Métiers" of Denis Diderot. Edited by Charles Coulston Gillispie. New York: Dover Publications, 1959. Two Volumes, Pp. 920, 485 Plates. $18.50.

Historians of technology have too infrequently used encyclopedias as source-books. Though books on certain arts and crafts were published from roughly 1500 onwards it is still difficult to penetrate the screen of "trade-secrets" which the guilds of artisans erected to their own distress; the help of science for which they clamored on various occasions could only be acquired by informing the scientist, but the latter had small incentive to apply his knowledge to arts and crafts with which he was little familiar. Still, if one compares these excellent sets of plates on agriculture, military arts, metallurgy, glass, masonry, carpentry, textiles, leather, papermaking and many other crafts with those given in James Truslow Adams's *Album of American History* or similar publications, their value is immediately proved to the historian of technology. In fact we know the details of certain trades only through such encyclopedias, which are truly characteristic of the eighteenth century.

Among the encyclopedias of that century, beginning with Chambers' two-volume *Cyclopaedia or Universal Dictionary of the Arts and Sciences* (1728), Diderot's work stands out, though even he had to resort to older illustrations taken from Agricola and Vesalius when he found too many tradesmen of his day inarticulate and unable to explain or put on paper the basic operations of their crafts. In the thirty pages of introduction, Prof. Gillispie ably explains how the publisher Le Breton set out to produce merely a French translation of Chambers' work but how d'Alembert (part-time editor) and Denis Diderot finally edited no less than 17 volumes of text (plus four supplements) together with 11 volumes of plates (plus one supplement) between 1751 and 1777. The introduction also tells of the revolutionary part played by this dictionary in French letters and philosophy and how Diderot used technology to preach his ideology of progress and liberalism. He helped to make his generation understand "science as power," the possibilities of Newtonian science as an instrument of thought and as an intellectual triumph, which finally led to the demolition of many social abuses rampant in the eighteenth century. Although many of his contemporaries were inspired by Newtonian science, Diderot's conception of science is drawn from Francis Bacon rather than from Newton.

In his *Advancement of Learning* Bacon had stated that "the use of history mechanical is of all others the most radical and fundamental

towards natural history; it shall not vanish in speculation, but such as shall be operative ot the endowment and benefit of man's life: for it will . . . minister and suggest for the present many ingenious practices in all trades." The Royal Society at its foundation planned to collect material for a survey of arts and trades; so did the Parisian Académie des Sciences (1675), and Réaumur prepared a set of plates to accompany the text which never materialized, though the first volume of plates was published in 1761. Diderot deserves our praise for having surmounted the obstacles of trade-secrets and jargon and having produced this most interesting survey of the arts and crafts of eigtheenth-century France. Prof. Gillispie may be right in stating that Diderot was more a clever publicist than a systematic thinker; however, for the historian of technology, the most important point is that he succeeded in producing such clear and effective illustrations of the technical operations and machinery used in his day. The charge of plagiarism flung at his head by many contemporaries may not be unfounded; for us the main thing is to have at our disposal this Encyclopaedia with clear descriptions of different trades, accompanied by some 2,900 plates, of which 485 have been selected by the editor for these well-produced volumes.

These plates give a host of operations, techniques, machines, tools, and processes, most of which can be traced by the use of the index. However, the reader should, if possible, consult the original Encyclopaedia, for the articles on the different trades illustrated by the plates produced here constitute the real contribution of Diderot and his collaborators; very often they are our only source for the study of a particular craft during the eighteenth century and in the days before the guilds were definitely dissolved. Such study is imperative for the historian of technology, though he may be as disappointed as your reviewer to find that in certain libraries Diderot's volumes, despite the importance of the text, are still partly uncut.

Generally speaking, the plates in the volumes under review are excellent in quality when compared with the originals. Our only quarrel with the publisher is that some double-page plates do not fold out but suffer from being bound, making it difficult to study the central part of the figure. On the other hand, this publication deserves the highest praise, for it will fill a real gap in our handbooks on eighteenth-century technology and craftmanship.

R. J. Forbes *

* Professor Forbes, of the University of Amsterdam, has written much on the history of technology, particularly in the fields of ancient and of petroleum technology. In 1959 Dr. Forbes was elected to Corresponding Membership in the Society for the History of Technology.

Contributions from the Museum of History and Technology, Papers 1-11 (United States National Museum Bulletin 218). Washington, D. C.: Smithsonian Institution, 1959. Pp. 201. 128 illustrations. $2.50.

Although many technical museums in England, Europe, and the United States have published official catalogues and guides to their collections for the general public, these museums, with very few exceptions, have not functioned as centers of scholarly research on the history of technology, even that based upon their own collections.

It is therefore a great pleasure to welcome the first in a series of monographic publications by the Museum of History and Technology of the U. S. National Museum. Recently roused from its slumber of nearly seventy-five years by congressional appropriations making possible a splendid new building and a greatly expanded professional staff, it now promises to rival the well deserved reputation of its sister institution, the Museum of Natural History. Under the energetic and scholarly leadership of Dr. Robert P. Multhauf and Dr. Philip W. Bishop we can have high hopes of seeing the Museum of History and Technology become the kind of technical museum which an industrial country such as the United States should have had long ago. The present volume marks an excellent first step in the creation of a center of scholarly research in the history of technology. It is to be hoped that this work will continue despite the pressure of curatorial duties connected with the transfer of the museum's collections and exhibits to their new home.

The various monographs in this volume illustrate not only the many fields open to the historian of technology, but a number of the approaches possible in their scholarly study. Some are quite specialized, such as Edwin A. Battison's paper on the "Auburndale Watch Company," or George B. Griffinhagen and James Young on "Old English Patent Medicines in America," or Grace L. Rogers, "The Scholfield Wool-carding Machines." Others, such as Dr. Bishop's "Beginnings of Cheap Steel," and Derek J. Price's "On the Origin of Clockwork, Perpetual Motion Devices and the Compass," deal with much larger and broader developments. Leslie J. Newville's paper on the "Development of the Phonograph at Alexander Graham Bell's Volta Laboratory" illustrates the use which can be made of archival material in the Museum's collections, while Dr. Multhauf's note on "Mine Pumping in Agricola's Time and Later" shows what can sometimes be done in the history of technology with numismatic materials. Edward C. Kendell's account of "John Deere's Steel Plow" and Donald H. Berkebile's "Conestoga Wagons in Braddock's Campaign, 1755" show how technological developments are entwined with other aspects of history. Jacob Kainen's paper, "Why Bewick Succeeded: A Note on the History of Wood Engraving," relates the history of

technology to the history of art, and W. James King in his splendid monograph, "The Natural Philosophy of William Gilbert and his Predecessors," ties the history of scientific ideas to general intellectual history. Although some divisions of the Museum of History and Technology are conspicuous by their absence, this is a most impressive array.

The papers of Messers. Berkebile, Kainen, Kendell, Newville, Multhauf, and Miss Rogers are all excellent and show the kind of work needed in the history of technology, where research must usually be based upon both documentary and museum materials. The other papers require or deserve more extended comment. The monograph by Griffinhagen and Young on early American patent medicines is well done, but seems hardly worth doing. Although the authors claim some value for their study, this is not borne out by their results. It is antiquarian rather than historical.

The account of the Auburndale Watch Company and its watches by Edwin Battison is based upon thorough research and illustrates the need for study of the technical details of the "hardware." But it suffers by Battison's presentation; he uses too extensively the highly specialized vocabulary of the watchmaker when a word or two of explanation at crucial points would have made his ideas clear to a much broader group of readers. On the other hand, his introductory paragraph is so vague that one cannot be sure if it is consistent with his conclusion. Battison's account would have benefited from substantial pruning of antiquarian irrelevancies, and better organization of the paper would have eliminated the confusion caused by sudden shifts from technical details to biographies of men concerned or to the growth of the watch company.

The most puzzling monograph in the collection is that of Derek Price on the origins of the mechanical clock. There are a number of statements which one would like to see more fully documented; and unless I have missed his point completely, he seems to have proved the exact opposite of his thesis.

It was not the weight drive, nor the complex gearing, but the *escapement* that was crucial for the first appearance of the mechanical clock. The further refinement of mechanical means of control of the weight or the spring drive was to remain the critical problem of the mechanical clock throughout its history. It was also this problem which made the clock significant for general technological advance, in the development of ingenious mechanisms and precision construction, and in the application of the methods and results of pure science to technology.

Although Dr. Price several times indicates the importance of the escapement, his argument ignores the fact that the problem of *control of motion in time* did not exist in most of the astronomical or perpetual motion devices he describes. It is also clear in the mechanisms Dr. Price analyzes which do have a method of time control, such as the Chinese, Alphonsine, and some others, that the ultimate means of control

was still that of the water clock and the sand glass—a restricted flow of water, mercury, or sand. Since these are clearly not *mechanical* escapements and since his other astronomical devices have neither weight drive nor any time control, one can hardly call any of them "protoclocks." One may properly call the sundial a kind of simple astronomical computer, and many of the devices he describes might better be called "protocomputers." But the evidence Dr. Price brings forward does make it quite evident that in the water clock there was, from antiquity to the end of the thirteenth century, only increasing complexity in its control devices and in their connection to a complex gearing not at all similar to that in astronomical devices. We remain in the dark, however, in our knowledge of when and how the transition was made from control of a time measuring device by a restricted flow of water, mercury, or sand to control by an oscillating mechanism.

Dr. Philip Bishop's paper is an example of the mature and careful scholarship which the history of technology so badly needs. Far too many legends and confused accounts have come down to us that must be cleared up before our discipline can be established on sound foundations. Bishop sets out to untangle the relationship of Martien, Mushet, Kelly, and Bessemer to early production of cheap steel. By masterly handling of the evidence he is fair to all, but he shows that we have been quite right in calling it the Bessemer process.

The most valuable monograph in the collection is W. James King's analysis of Gilbert's work on magnetism. With meticulous scholarship, excellent documentation, wide erudition, and philosophical perspective, he shows that, as in the science of mechanics in the seventeenth century, many of the concepts of Gilbert's new theory of magnetism had their origins in byways of medieval theology.

The volume is profusely and well illustrated, but the design of the book is not at all up to the standards of many recent publications of the Government Printing Office. Furthermore, one wishes that an index had been provided for the volume, as well as bibliographies for each of the monographs. There might also be some concern for the bibliographic wisdom of including such a diversity of monographs in a single volume, were it not known that they are being reprinted separately. The articles were all completed in June of 1957; this sort of delay in publication is not encouraging to further scholarly work, despite the generally high standards of this collection.

ROBERT S. WOODBURY *

* Professor Woodbury of the Massachusetts Institute of Technology is an authority on the history of machinery.

Risk and Technological Innovation: American Manufacturing Methods during the Nineteenth Century. By Paul W. Strassman. Ithaca, New York: Cornell University Press, 1959. Pp. x, 249. $4.00.

Most scholars hope to produce a book that will be both stimulating in its findings and conducive to further research. In this short essay Mr. Strassman almost certainly has achieved both these objectives.

The author set out to ascertain the interaction of business enterprise and technological change, exclusively in connection with innovation. To do this he tested the pertinent theories of Schumpeter and Veblen with data from the history of the iron and steel, textile, machine tool, and electric power industries. Schumpeter believed leading American entrepreneurs to be creative and that such innovators took heavy risks. Veblen admitted that "leading businessmen might have risked much in their financial maneuvers" but insisted "that they risked little or nothing in promoting the serviceability of an industry." In addition, he maintained that, particularly after 1860, the role of the American industrialist "with respect to technology was at best permissive, not creative, although there were 'comparatively slight and infrequent' exceptions." Mr. Strassman has stated his conclusion on page 4: "The leading promoters of new manufacturing methods were . . . a creative factor in economic development; but, by and large, they proceeded with a degree of caution that, with certain exceptions, reduced the chance of error to negligible proportions."

An outstanding feature of Mr. Strassman's analysis is the care with which he defined certain concepts and terminology. For purposes of the study the definition of innovation included "only experiments and inventions financed by entrepreneurs for proposed innovations." (p. 11) The term "risk," the author recognized, in the "actuarial sense" is a "knowable parameter of a frequency distribution," but he used the word to mean "a significant but to some extent unpredictable chance of loss." (p. 11) He systemmatically categorized risks of innovation under four headings—production risks, customer risks, interference risks, and risks of timing. For a clear understanding of meanings and classifications in connection with types of risks the reader should examine pages 13-15 of the book.

A term that Mr. Strassman might well have defined carefully, since he has used it so extensively, is "caution." During the nineteenth century innumerable businessmen utilized the word synonmously with "prudence." But actions that seemed "cautious" and "prudent" to one entrepreneur seemed the wildest sort of gambling to another. In evaluating the caution of an innovator, perhaps Mr. Strassman should have weighed more carefully the relation between capital already invested in the firm, plus attainable credit, and the estimated cost of the projected innovation; a small iron manufacturer, with limited credit

and knowledge of Kelly's failure, would seem to have reason to be frightened of risking his "all" on the Bessemer process in 1860.

A few of many other queries should be mentioned. Has not Mr. Strassman, by definition, specifically excluded from consideration a fundamental part of the innovative process, thereby giving a bias to his entire study? In his own words, he was "not concerned with the chance of loss, sustained as part of the process of technical change, by inventors in garrets, employees, or the community at large." (p. 11) In most instances, the false starts and early failures are not considered. This is exactly the period in the history of almost every invention when risks were greatest and losses highest—when would-be innovators paid the price of progress or of not being cautious enough. To raise another type of issue, should not a scholar testing the concepts of Schumpeter and Veblen put his study in the broadest possible social, economic, and political context and check the validity of basic assumptions during the course of his research?

For many teachers the best part of Mr. Strassman's book will be the chapters on technological changes in the four selected industries. The essays constitute about as neat syntheses of mechanical innovations in the iron and steel, textile, machine tool, and electric power industries as can be found in print. Without doubt, professors will be assigning one or more of them for supplementary reading in courses on the economic as well as the general history of the United States. At the same time many will surely hope that Mr. Strassman, or some other scholar, will feel impelled to write similar essays on other industries.

That some additional students will respond to the challenge of testing Schumpetrian theories with data from the history of other nineteenth century industries is to be fervently hoped. Mr. Strassman has shown the way in his fashion; others should see if his methods can be improved and if their own conclusions for other industries are comparable to those found by this one author for the four areas examined.

RALPH W. HIDY*

Steam's Finest Hour. Edited by David P. Morgan. Milwaukee: Kalmbach Publishing Company, 1959. Pp. 128. Illustrations. $15.

This impressive volume presents on its sixteen by eleven-inch pages a pictorial record with accompanying explanatory text of 101 of the finest examples of steam locomotives employed on the railroads of the United States, Canada, and Mexico during the last quarter-century

*Dr. Hidy is Isidor Straus Professor of Business History at the Graduate School of Business Administration of Harvard University. He is the author of *History of Standard Oil (New Jersey)* and of other studies in business history.

of the steam locomotive's development and general use. In the years following World War I the locomotive builders and their customers, the railroads, began an extensive program of research and development devoted to the improvement of the power and efficiency of the steam locomotive. This period, extending from the 1920's to the present when the steam locomotive has almost completely relinquished its role as prime mover, may be called the period of the "modern" steam locomotive in America. Such developments as the mechanical stoker, increased firebox size and boiler capacity, improved counterbalancing of wheels and other moving parts, pressure-type lubrication systems, and the use of roller bearings on all wheels made possible the construction of progressively more powerful and efficient locomotives with emphasis both on horsepower and speed.

The editor of this work, Mr. David Morgan, who serves also as the editor of *Trains* magazine, separates his representative locomotives into nine groups. Seven of the groups are identified by the general geographical region of the United States in which the locomotives were used in road service. An eighth group comprises the representative types of Canada and Mexico, and the ninth group is devoted to the engines of the switching yards. Thus in the New England Region the "Berkshire" type, perhaps the most popular design to emerge in the modern period, received its first extensive adoption by the Boston and Maine and the Boston and Albany railroads. At the same time the smaller railroads in more modest circumstances made do in the traditional New England manner with modernization of older locomotives or the purchase of small numbers of new ones. In the Eastern Region such trunk lines as the New York Central, Pennsylvania, and the Baltimore and Ohio pressed the development of the super steamers needed for the fast passenger and heavy freight traffic of the nation's New York-Chicago artery. The connecting lines and short lines of the East were no less aggressive in their efforts to develop motive power more and more capable of handling the coal and ore traffic of the Great Lakes-Allegheny regions and the transfer traffic along the Eastern Seaboard. The railroads of the Middle West Region were bothered little by mountain grades and could concentrate on fast passenger running and serving freight traffic with a large seasonal farm produce component. The "Northern" type used for both freight and passenger service enjoyed its widest acceptance by the railroads of this region. The Midwest Region also witnessed one of the first instances of diesel competition when the Milwaukee Road developed its hundred miles-per-hour plus "Hiawatha" locomotives to race with the Burlington's dieselized "Zephyr" on the Chicago-Twin Cities run.

The coal roads of the Pocahontas Region, Chesapeake and Ohio, Norfolk and Western, and the Virginian, developed steam power "to the screaming point" in their efforts to haul tremendous volumes of bituminous coal from the West Virginia mountains eastward to the

Tidewater and westward to the blast furnaces and mills of the lower Great Lakes area. In this coal-filled region steam power survived in general use longer than in any other. The Southern Region produced few new locomotive designs after 1930 because its primarily agrarian economy up to the time of World War II provided less of the traffic density which in other parts of the country stimulated the design of big steam power. Large numbers of new locomotives were acquired in the modern period only by such trunk lines as the Illinois Central. Other roads tended more to purchase small numbers of locomotives of design already proved elsewhere. In fact, the Nashville, Chattanooga, and St. Louis Railroad acquired five "Northern" type locomotives modified to suit its own particular needs, including of course the change of type name to "Dixie." The Southwest Region is best known for the development of the "Texas" type, a straightforward elongation of the well-established "Berkshire," and the highly individualized development of "Northern" types by such roads as the Santa Fe, Southern Pacific, and Missouri Pacific.

The Western Region with its great distances, formidable grades, and winding routes through mountainous terrain made the biggest demands for locomotive power. Fortunately the traditional spaciousness of the West provided ample clearances along the rights-of-way to accommodate the monsters designed for service in this region. Union Pacific's "Challenger" and "Big Boy" classes, Santa Fe's oversize "Texas" and "Northern" classes, the Great Northern and Northern Pacific versions of the articulated locomotive, and the Southern Pacific's "Coast Daylight" streamliners and cab-forward articulated locomotives epitomized all that was powerful, imaginative, and exciting in steam locomotive construction.

Steam's Finest Hour presents for each locomotive type a builder's profile picture, a data table giving design and performance specifications, and additional pictures showing the locomotive in operation. This wealth of detail is embedded in a continuity of text which maintains well its balance between engineering detail and nostalgic lore. The body of data here presented in word and picture is invaluable to anyone interested in railroad transportation during the period extending from World War I through World War II to the present.

Regrettably, most of these proud beauties have passed not only into disuse but further into oblivion under the cutter's torch. Some are preserved for posterity as museum specimens and some others wait, mothballed by their owners, in anticipation of peak period demands for motive power. The passing of the steam locomotive from general use carried into history one of the most romantic and exciting features of the American scene over the past century. In the opinion of this reviewer, the greatest contribution of the volume at hand lies in its lavish presentation of this segment of Americana. Certainly no youngster of nineteen or ninety will peruse this book without once again

examining the proposition that to have been a steam locomotive engineer would have been one of life's greatest delights.

PAUL E. GUENTHER *

Oil on Stream! A History of Interstate Oil Pipe Line Company, 1909-1959. By John L. Loos. Baton Rouge: Louisiana State University Press, 1959. Pp. xii, 411. Maps, illustrations, notes, bibliographical note, and index. $5.00.

The recent centennial of the petroleum industry stimulated a huge output of articles, monographs, and books. If some reached a respectable level of journalistic achievement, significant contributions to petroleum literature were disappointingly few, for exploitation of new source materials or developments of new interpretations of known materials were rarities indeed. One such exception is the present volume, which merits the attention of general readers as well as of economic and technological historians and other specialists.

Professor Loos has given us the first integrated history of an early (1909-1959) pipeline system tapping the mid-continent region. No more revealing setting for such a study could have been found, for this great source of crude oil supply had a tremendous impact on the patterns of ownership and competition in the petroleum industry during the first three decades of this century. The author obviously has dug deeply and well into previously unexploited materials in the archives of the Interstate Oil Pipe Line Company and other affiliates of Jersey and Louisiana Standards. If his materials sometimes resisted answers, he has not neglected asking questions of them ranging beyond the parochial viewpoint of an individual company.

The interplay of technological evolution, management and labor relations, and operating procedures forms a substantial part of this book. The fascinating technological evolution toward mechanization and even, in recent years, toward automation is spelled out with accuracy and clarity. If that unfolding technology was largely derivative and lacking in major innovations, the very gradualism of its development and application makes it elusive and difficult to appreciate without such an informative account. Instructive too are the indications of the ways in which localized terrain conditions, as well as shifting objectives of management, modified the speed and nature of technological adaptation. In short, though the broad outlines of pipeline technology may be simple, there is a deceptiveness in that sim-

* Dr. Guenther is Associate Professor of Mathematics at Case Institute and a long-time student of railroad history.

plicity. The actual progression of that technology has much more subtle patterns than is generally appreciated.

Early phases of this evolution perhaps hold the greatest interest because we are less familiar with them. In 1909 telegraphic communications had been a common component of pipe lines for many years; a recent supplement at pumping stations was the telegraphone, a low-powered telephone permitting voice communication. Pipe, usually six-inch on trunk lines, was lap-welded and laid in 20-foot sections by tong gangs. Not only terrain and available horsepower but density and related characteristics of the crude oil determined distances between pumping stations. Somewhat surprisingly, Louisiana practice had already embraced air injection semi-diesel engines with one cylinder and 85 h. p. Partly because of road conditions, use of the automobile in field work, on the other hand, lagged until about 1915. Not until two years later did the first trucks appear in Oklahoma portions of the line, equipped with solid tires, carbide lights, buckboard seats, but no windshields. The same year saw the introduction of the first pipe-laying machine, but as late as 1924 this phase of operations still resisted mechanization in rugged mountainous terrain.

The first major advance against the ever-present menace of corrosion was made in 1915 with the slow and expensive operation of cementing the pipe. Less than a decade later asphalt coating, applied without interruption of the flow through the lines, fell short of its seeming promise. But soon thereafter a more sophisticated coating proved so satisfactory as to be continued into the 1940's, when electrolysis and other techniques came to the forefront of corrosion defense. Digging ditches for new lines and to uncover existing pipes began to succumb to mechanization only in 1926 with the introduction of ditching machines for stripping the earth off the pipe.

As might be expected, welding advances awaited developments in other branches of the industrial arts. Welding with acetylene gas, developed in World War I, was first applied in 1920, but only for the repair of broken engine and pump parts. Tried four years later in building a pipe line, it was found to be too brittle. Not until the early thirties was electric welding by gasoline-driven machines successfully applied on a broad scale.

Such an abbreviated sampling at best gives an imperfect impression of the level of technological treatment in this book. The author develops several other important themes with equal care: the evolution of administrative techniques and corporate structure; changing elements in decision-making; managerial responses to altering sources of crude supply and demand, and most especially to the flux in the legal and regulatory environment. These complexities do much to explain the subordination of technological considerations in pipeline operations throughout much of this century and the rarity of major innovations originating within pipeline systems.

This work has importance even if viewed only as a case history of a pipeline system that expanded to 4500 miles over eight states. But Professor Loos by his treatment has realized an importance much beyond that: he has made a notable contribution to petroleum literature.

ARNOLD R. DAUM *

The Functional Tradition in Early Industrial Buildings. By J. M. Richards. London: The Architectural Press Ltd., 1958. Pp. 200. 265 photo illustrations by Eric de Maré. $5.05.

In past generations the literature and the academic teaching of architecture centered on archaeological studies; it followed that building design adopted eclectic patterns. But in recent decades the architectural educators have minimized emphasis on the past and have taken a pragmatic approach with contemporary as their idol and "functionalism" their watchword. Some archaeological studies are continuing, but, like the other specializations of our culture, they are generally set apart from the current trends in architectural teaching. In the eyes of most students and many educators today, contemporary sprang full blown from Louis Sullivan and Frank Lloyd Wright in America and from Peter Behrens and Walter Gropius in Europe; the present attitude is that contemporary has no past.

Recently, however, a few scholars in the architectural field have been digging into the work of the nineteenth century; and they are now finding that contemporary, too, has a past. The outstanding monumental buildings of the Renaissance, strong on aesthetics and social impact but unimaginative in structure, were largely the work of great creative individuals whose names are familiar to us. Later, as the industrial revolution progressed, technology entered more and more into the building process and the very lives of men. As Abbott P. Usher points out in his *History of Mechanical Invention*, "The achievement of the inventor and technologist . . . is merely a contribution to a flowing stream of accomplishment in which the identity of the individual work is obliterated by all of the natural circumstances of social life."

Obviously this book, *The Functional Tradition*, was prepared with these viewpoints in mind. Mr. J. M. Richards, Executive Editor, the *Architectural Review* magazine, London, and author of several excellent books relating to contemporary architecture, together with the architectural photographer, Mr. Eric de Maré, ably presents a review of the functional aspects of those little known anonymous

* Dr. Daum is Associate Director of the Petroleum History Project at Northwestern University.

structures that were developed in England to meet the needs of the rising industrial revolution. The book is largely pictorial with ample descriptive captions on every page. In addition to the eight pages of excellent introductory text, there is a leading text for each of the chapters: Warehouses, Docks, Naval Dockyards, Textile Factories, Water Mills, Windmills, Breweries, Sheds and Bridges.

In the introductory text Mr. Richards discusses the connotation of "functionalism," its relation to the historic past and to the recent contemporary movements in architecture. He then proceeds to point out that these early industrial structures were functional and made good use of the materials and building methods then available. In his words, they "derive their artistic quality from the way the challenge is met. . . . The buildings illustrated here show the wide range of aesthetic effect, and the subtlety in form and detail, of which an architecture dominated by function is capable."

The development of these structures related to industry is deeply rooted in the cultural pattern of England during the period of the industrial revolution. In the earlier agricultural economy the building craftsmen were quite skilled in the handling of stone, brick, and timbers, particularly in buildings of domestic scale. But here new problems arose, problems in the construction of huge textile mills, multistoried, requiring large open loft spaces. In the same decades that Robert Adam and John Nash were receiving great acclaim for their designs of sophisticated Georgian town houses, these anonymous builders in the new industrial areas were creating significant new forms of new scale to meet the functional requirements set out by the mill owners.

This was an age when the development of concrete was in its infancy, the use of cast iron was still experimental, and steel was prohibitively expensive. The earlier factories, for example, the Calver Mill (Derbyshire, 1785), were interesting expressions of traditional load-bearing walls pierced with regularly-spaced window openings. The simple rhythmic, undecorated fenestration patterns in the large masonry masses foreshadow the simplicity of more recent contemporary structures. Later the internal structure of cast and wrought iron supplemented the exterior masonry walls; then beginning in the mid-nineteenth century the industrial builders pioneered in the complete iron frame structure almost thirty years before William LeBaron Jenney built his first Chicago skyscraper using a frame of steel. Most of these buildings were simple expressions of their materials and structure. In those structures where decorative architectural features, such as Palladian windows or Gothic tracery, were introduced the effect is quite superficial.

The author traces an interesting evolutionary development of the horizontal strip window now so prevalent in recent commercial and industrial buildings. Before the large textile mills were developed, weaving and spinning were carried out largely by families and indi-

viduals in their own homes. In the upper story work rooms, the grouped openings, known as "weaver's windows," were provided to give ample light. A cottage at Golcar, Lancashire, is illustrated. Then in the larger mills, for example, the Stanley Mill (Stonehouse, 1813), bands of horizontal windows were constructed in the exterior masonry walls. The pattern is carried to a more complete development in the Boathouse at Sheerness Naval Dock built in 1861; here the cast iron frame makes possible a grid pattern with dominantly horizontal fenestration that is quite contemporary in effect. Were designs of Sullivan of America and Gropius of Germany influenced by these structures?

The text, together with the well selected photographs, presents new insights into the usual interpretation of functionalism and its relation to the contemporary. Although the text and photographs are excellent, the graphics and format are not well designed for this type of book. The jacket, with its Victorian type lithograph and lettering, is not an invitation to further reading. Inside, the masterful photographs often are not adequately presented; the full page photos *almost* fill the page, but not quite. The frontispiece, with the photograph filling the entire page, shows the possibilities that were missed.

For the scholar interested in the historical background development of contemporary architecture and its relationship to our technical culture this is an excellent source book, but it may have little influence on the busy, commercial-minded architectural practitioner.

MELVIN M. ROTSCH *

Philosophy of Structures. By Eduardo Torroja. English version by J. J. and Milos Polivka. Berkeley and Los Angeles: University of California Press, 1958. Pp. vii, 366. Illustrations. $12.50.

During the past century and a half the structural arts have undergone a revolution that has radically transformed the entire traditional character of building. Perhaps the most important technical consequence of the Industrial Revolution of the eighteenth century has been the progressive symbiotic unification of science and technology, a union which has simultaneously placed technology on a scientific basis and turned the scientist increasingly toward technology as a fertile field for inquiry and discovery. Building techniques have always been the slowest to respond to technological innovation, but by the end of the nineteenth century these too had come to be drastically reshaped by the application of the methods and conclusions of modern

* Professor Rotsch teaches the history of architecture at the Agricultural and Mechanical College of Texas and has published articles in the fields of landscape architecture, city planning, and architectural history.

physical science. By the mid-twentieth century all large-scale structures clearly revealed that building had been transformed from an empirical and pragmatic craft to an exact science characterized by a well-developed structure of theoretical knowledge, by predictive power, and by elaborate experimental methods. The chief tools in this unified intellectual-technical evolution have been accurate methods of stress analysis, the theory of elasticity and strength of materials, the investigation of the internal structure of materials, and the appropriate mathematical instruments.

The nineteenth century saw the introduction and mastery of iron and steel framing and of reinforced concrete in arches, frames, and slabs. The major innovations of the present century have lain in the direction of the increasing lightness, strength, and precision of structural elements. Welded frames and trusses, extreme light-weight frames such as geodesic domes, and suspended construction by means of high-tensile wire cable characterize a great proportion of steel structures. More revolutionary for both the economic and formal aspects of building has been the development of thin shells and plates and of prestressed members in reinforced concrete. The result has been an openness, buoyancy, and continuity of structure wholly unprecedented and, up to little more than a generation ago, simply unimaginable in traditional building art. The visual consequences of these techniques are often spectacular. The new forms are enthusiastically adopted by all the advanced architects, whose productions are regularly and widely publicized in exhibits, books, and the popular press. Except for technical treatises beyond the comprehension of the layman, however, few of these publications have had to do with the science and technique of modern structure. The engineers have been too busy designing to tell us of their work, and the architectural critics and historians lack the requisite knowledge.

The present book is an impressive example of how ably the story can be told by a highly literate engineer given to philosophical reflections on the character and function of his art. Eduardo Torroja is one of the great creative builders of our time, one who has achieved international fame for the design of reinforced concrete bridges and buildings erected in his native Spain. *Philosophy of Structures* is a revised and expanded English version of his earlier work on modern structural techniques, originally published in his own country under the title *Razón y ser de los tipos estructurales*. The later work represents the most thorough and lucid attempt yet made to provide the layman and the student with an understanding of the complex functional character of contemporary structures and their antecedents in the past. Certain passages of the presentation may try the patience of the reader, but for the most part his effort will be well rewarded.

Señor Torroja begins with a broad inquiry into the nature, the functioning modes, and the utilitarian and aesthetic purposes of construction,

after which he plunges directly into an analytical treatment of his main subject. The phenomena of stressing—that is, the various aspects of the internal reaction of a member to the loads it must sustain, are presented first in terms of external equilibrium and internal resistance, then according to the fundamental kinds of stress, tension, compression, and shear. From this basis he takes up the traditional and modern materials of construction, brick, stone, and timber, and steel and concrete. Through a perfectly logical development of his subject, he moves from materials to basic structural forms—bearing walls and foundations, arches, vaults, and domes, beams and slabs, and trusses—and thence to the larger types of structures—dams, buildings (divided into floor and covering), bridges, and aqueducts. His analysis of the character and role of structural elements is distinguished by a most careful presentation of the organic union in a properly designed work of the physical properties of the particular material, the function which the member can thus be expected to perform, and hence its appropriate role in the whole complex of the structure. The examination of this unity, which characterizes the scientific use of modern engineering materials, is redeveloped in a general essay and forms the concluding chapter of the technical portion of his presentation.

Two chapters on the aesthetics of contemporary structure are likely to prove most interesting to the technically untrained reader who is acquainted with current architectural work. The author belongs to the school which holds that beauty is the natural outgrowth of the empirical rightness of structural form. His exposition of this doctrine is the most thorough and least confused of any that this reviewer has seen, although the idea has been repeatedly developed in a great many histories and critical essays pertaining to the modern movement in architecture. Señor Torroja's analysis of the formal qualities of contemporary structure, those patterns of line and surface that arise from precise associations of structural elements, constitutes an aesthetic theory of abstract and purely geometric forms. Within its limits this theory is valid and not open to serious question. The confusions in modern architectural practice and its associated philosophy of form arise from an unwarranted extension of the aesthetic of empirical form to the art of architecture in its totality. The question is whether the abstract formalism of modern structure, however far it transcends utilitarian demands, can be equated with architecture conceived as a symbolic art.

The two final chapters of Señor Torroja's book are in part a guide to the novice in structural engineering—what he must know, how he may learn, his place in the designing organization. In his discussion of the genesis of the structural scheme the author gives us some insight into how a creative engineer works, and here we see the extent to which intuition and imagination enter into the process of design. Exact calculation of loads, stresses, and dimensions of members comes

only after the problem has been conceived in its entirety and the structural solution has taken shape in the designer's mind and to a considerable extent on paper. This intuitive grasp of the many variables that compose the problem and the possible structural forms that can be created to meet them is something that can come only from wide and accurate knowledge of the art. It can no more be taught than can creative imagination itself; it arises only from the steady accumulation of experience.

For its difficult subject, Señor Torroja has written a remarkably effective book; yet the general mode of presentation may sometimes prove baffling to the reader who is wholly unacquainted with structural theory. The author's style is in places oddly elliptical, while in others it bristles with a formidable technical vocabulary. Although the publisher recommends the book for laymen, and this reviewer concurs, the author would have made his fascinating and culturally important subject more comprehensible if he had begun his presentation on a more fundamental level. This is especially true of his second chapter, "Phenomena of Stressing," which provides us with a good introduction to types of deformation and failure and to isostatic patterns, or stress distribution, but which conspicuously lacks a clear explanation of the nature of stress itself and of such related phenomena as elasticity in general, elastic limit, ultimate strength, and stress-load relationships.

Aside from minor typographical defects, there are a few errors in the book, but only one is worth making an issue of because of the injustice it does to an American pioneer in the development of modern concrete. On page 251 the Alvord Lake Bridge (1889) in San Francisco's Golden Gate Park, the first reinforced concrete bridge in the United States, is ascribed to the Austrian engineer Josef Melan and is given as an example of Melan's system of reinforcing with heavy steel ribs. The designer, however, was Ernest L. Ransome of San Francisco, and the bridge is reinforced with thin bars disposed in a system which is the basis for the now nearly universal technique of reinforcing concrete structures. But the weaknesses of *Philosophy of Structures* are minor factors in the general excellence of the work. All who are interested in building technology must be grateful that Señor Torroja has taken the time from a busy career as a designer to give us this profound and illuminating essay on his intricate art.

CARL W. CONDIT *

* Dr. Condit, Professor of General Studies at Northwestern University, is author of *The Rise of the Skyscraper* and the recently published *American Building Art: The Nineteenth Century*.

Edison: A Biography. By Matthew Josephson. New York: McGraw-Hill, 1959. Pp. xii, 511. $6.95.

This is an outstanding biography because Matthew Josephson has the unique talents necessary to encompass the depth and breadth of Edison. Not only was Edison an inventor, but he was an inventor sensitively attuned to the economic environment in which he lived. Nor was Edison a commonplace personality; he was compounded of the subtleties productive of genius. Josephson, a biographer of great men (Rousseau, Zola, and Hugo) and a student of economic history (*The Robber Barons*), has brought his abilities into play in this biography.

Edison invented for an economic market. He invented for the Western Union Company and for Jay Gould; he invented for such great financiers as the Vanderbilts and the Morgans; and he established a research laboratory to make invention a business (but he did not invent to make money). Josephson knows the financiers and he knows the economic milieu of the late 19th century. He is able to describe the web of finance and invention that made up the complex development of the automatic printing telegraph and the multiplex telegraph. He is sensitive to the shifting circumstances of capital and competition to which Edison was exposed. He can describe, for example, the feverish speculation in gold in 1869 that served as a stage for young Edison's dramatic repair of a gold price indicator when its failure paralyzed frantic financial offices. Josephson, in short, establishes the developing relationship between finance and invention which constituted the basis for Edison's success.

Josephson also has the psychological insight to fathom Edison in his dual role of husband and father. He knows that Mary Stilwell, Edison's first wife, loved him but did not have the will nor the comprehension to influence significantly his personality or his career. On the other hand, Edison's second wife, Mina Miller, emerges as the consort of the great man—gently moulding the impetuous "tramp" telegraph operator turned inventor into a more conventional image of wealth and success. The failings of Edison the father are not overlooked.

Josephson, then, has portrayed the man in a complex which extended beyond the horizon of pure invention. How well has he recreated Edison the inventor? The author has mastered the substance of Edison's inventive activity and has written of it quite authoritatively and vividly. He has drawn upon excellent monographs, such as Harold Passer's study of the electrical industry, and has supplemented these with research in the extensive holdings of the Edison Laboratory National Monument in West Orange, New Jersey. He effectively describes Edison as an improver of technology when he worked on the gold indicator, the automatic printing telegraph, and the multiplex

telegraph. He makes it quite clear that Edison drew upon his first-hand knowledge of the telegraph system to work the "bugs" out of existing inventions and to modify others. He follows the invention of the phonograph and telephone transmitter step-by-step and analyzes the "problems" and "solutions."

Josephson understands that Edison was not working in a technological vacuum and that "invention is not so much bred by old Mother Necessity as by a growing reservoir of knowledge of various arts and industries, by the rising level of culture and technics" (p. 62). He refuses to judge Edison "the great tinkerer" but stresses his power of observation, his imagination, and his clear-cut reasoning faculty. Josephson is not misled by Edison's own remarks into portraying the inventor as anti-scientific, but takes into account the fact that he employed mathematical physicists (notably Francis R. Upton), trained chemists and metallurgists, and even obtained the help of university professors.

Josephson devotes considerable attention to Edison's best known inventive enterprise—the incandescent light system. In analyzing the inventive process in this case—and Edison was an innovator as well as an inventor—the author stresses Edison's thorough study of the prior literature, his search for parallels in the existing gas system, and his boldness in discarding the reasoned subtleties of "authority" if it contradicted the logic of fundamental scientific laws (Ohm's law in the case of his electrical system), or his experience (with the electrical system of the telegraph), or experiments (of his large staff at his Menlo Park laboratories). Josephson properly emphasizes Edison's concept which resulted in the invention of a system of generators, incandescent lights, distribution lines, and related apparatus, rather than in simply the invention of the electric light.

Having written an exciting and informative account of Edison's most productive years culminating in the Pearl Street central station, Josephson then describes the disillusionment following Edison's loss of influence in the Edison General Electric Company—it grew rich on his inventions and then struck his name from the title (1892). The narrative of invention is rounded out with the abortive ore separation ventures, the pioneer motion picture work, and the inconclusive storage battery venture at the new laboratory in West Orange.

The reviewer, then, cannot say "Edison has not yet found his biographer," for the book is worthy of the man; it should dispel lingering doubts that Edison was a great man. The biography, however, should not discourage work on Edison. Josephson drew upon the primary source material of the Edison Monument at West Orange, but he by no means exhausted about a quarter million items and 3,400 notebooks. Another synthesis is not needed until such primary sources as these are more fully exploited.

The publication of this biography should especially not discourage

further study of Edison the inventor. While Josephson has given a narrative of the inventive process and while his interpretation of it is sophisticated, he has not explained it (nor did he aspire to, judging by the character of his book). The Edison inventive process will only be explained when the technological environment in which he worked is better understood. Josephson knows what Edison did, but he is not able to put this into the perspective of what other engineers and scientists were doing and had done, so that Edison's work can be compared and contrasted. He does not tell us what Edison learned from the scientific journals he digested so thoroughly; he does not tell us of the existing state of the art of which Edison was aware—for example, prior work in constant voltage dynamos or parallel circuits which Edison must have drawn upon. Until Edison is placed in the technological environment as Josephson has placed him in the economic, there is still much more of significance to learn and write about Edison the inventor.

THOMAS P. HUGHES *

Science and Technology in Contemporary War. By Maj. Gen. G. I. Pokrovsky. Translated by Raymond L. Garthoff. New York: Frederick A. Praeger, 1959. Pp. ix, 180. Illustrations. $4.50.

More than anything else, one needs a sense of proportion when reading General Pokrovsky's book, or, more precisely, collection of writings. Pokrovsky is an astute, knowledgeable Soviet military man who has a feeling for the technology of warfare and a flair for publicizing it. On the other hand, he is not the oracle or the principal spokesman for Soviet military technology nor the key to solving the riddle of what the Kremlin plans to do with its highly sophisticated and destructive military gadgets. If he did all the things which some American observers of the Soviet military scene credit to him, he would be the wizard of the age. In sum, Pokrovsky contributes a valuable insight into Soviet thinking on the role of technology of warfare, but he is not the high priest of Soviet military technology, reading the gospel to the Soviet military congregation.

There is much of interest in Pokrovsky if one disregards his frequent lapses into propaganda about the superiority of the Soviet system for stimulating technological progress. However, it may be more profitable first to explore two of his fundamental misconceptions which indicate how grossly he sometimes misreads the modern technological military environment.

* Dr. Hughes, Associate Professor of History at Washington and Lee University, has written much on the history of the electrical industry in the United States and in Germany.

First, Pokrovsky's contention that nuclear explosives carried in long range delivery vehicles have not displaced conventional mass armies as the core of military strength is as unrealistic as Don Quixote's bout with the windmill. Khruschchev's recent acknowledgment that Soviet military strategy increasingly will depend upon nuclear weapons systems (and a concomitant reduction in conventional forces) shows how far Pokrovsky is out of step with his own decision makers. Pokrovsky fails to realize that as man designs and applies new weapons, he increasingly builds systems around them until he finds that he has little alternative but to use them in a showdown. The U. S. and U. S. S. R. have committed themselves to nuclear weapons, have devised their systems to support a nuclear attack, and, therefore, have relegated their conventional weapons to use in special situations and supplementing functions.

Second, surprisingly enough, Pokrovsky fails to grasp some of the major technological facts of life because he does not even think like a good Communist. He may loudly claim that dialectical materialism alone provides an understanding of nature and man, but he neglects dialectics in analyzing the relationship between technology and human direction. He echoes the popular misconception that technology's effect upon military strategy is a one way street. Analysts today seem preoccupied with the *impact* of technology on strategy. If Pokrovsky were to reread his Hegel he might see that technology and military strategy have a dialectical relationship; they act and react on one another, generating new forces which repeat this process. Military technology allows us to follow certain strategies, while the adoption of certain strategies stimulates the development of specific technologies. For example, although much of military strategy today revolves around missiles, it was Hitler's belated acceptance of the strategic thinking of missile warfare advocates and his subsequent support of Germany's missilemen that ushered in the missile age.

Let us turn now to the asset side of the ledger. There have been few translations of Soviet military writings; thus we are indebted to Raymond Garthoff for translating Pokrovsky. Further, Pokrovsky's writings represent a cogent effort to educate Soviet military men as to the intimate relationship between science and technology and the conduct of war. We could profit by this same lesson. He analyzes the significance of various fields of knowledge, such as mathematics, physics, chemistry, and biology, to military affairs and is wise enough to make mention of social sciences. He clarifies complex phenomena of atomic and space science, pointing out significant military applications. And he does not neglect the changes in the organization of military forces which must parallel the development of weapons.

Pokrovsky rightly sees that "practical war experience of the past can now be looked upon only as an important historical signpost of the past and not as some unchanging dogma determining military

affairs of today and tomorrow" (a position which conflicts with his argument for conventional mass armies which fought yesterday's wars). One could say that the rate of change in military operations is directly proportional to technological development; with today's exponential rise in the rate of change in military technology, previous wars offer less and less guidance for conducting future conflicts.

If anything, Pokrovsky does not drive this point home hard enough; he should carry his argument to its logical conclusion. Since military leaders can obtain little guidance by perusing history, they increasingly must rely on plans of action especially tailored for fighting future wars, namely, a technological strategy. A technological strategy is the planned action of applying technology to solve military (and non-military) problems, as contrasted to military strategy, which is concerned with the employment of technology, manpower, organization, and other resources to attain military ends. The technological strategist is concerned as much with accelerating the accumulation of scientific and technological knowledge as with applying that knowledge to achieve military objectives. Both American and Russian readers should come away from Pokrovsky with the realization of what technology can do for military operations. In brief, the student as well as the practitioner of military and technological affairs will welcome Pokrovsky's writings for the insight he gives to Soviet thought on a subject which shapes our destiny. They also should profit by Pokrovsky's errors and by his omissions.

RALPH SANDERS *

* Dr. Sanders, currently with the Industrial College of the Armed Forces, has taught at Dumbarton College and the American University and is associated with the Foreign Policy Research Institute of the University of Pennsylvania.

Communications

REPLY TO MR. MARTIN BURGESS ON NAIL-MAKING METHODS

Editor's Note: Dr. Smith's metallographic note on "Methods of Making Chain Mail" (*Technology and Culture*, I, 1 [Winter 1959/60], pp. 60-67) threw down the gauntlet to Mr. Burgess, of the Department of Egyptology of the University of London, who accepted the challenge in his "A Reply to Cyril Stanley Smith on Mail-Making Methods" (*Technology and Culture*, I, 2 [Spring 1960], pp. 151-55). The jousting continues herewith.

I am delighted that my note on mail (no longer "chain-"!) has elicited this critical comment from so eminent an authority as Mr. Burgess. My approach was of necessity limited to the findings of metallography on a few samples, and such laboratory studies need to be placed in context by scholars who are concerned with broader aspects of the problem.

I share Mr. Burgess's opinion about the foolishness of confusing tweed and silk (though one may discuss both, and sometimes compare them to advantage) and I will not defend the dates assigned to the samples I examined: a poor metallographer can merely read museum labels and take all stylistic dating with a degree of skepticism. (There was a typographical error in the attribution of Sample No. 14 [page 64]: it was from the famed Walters Art Gallery of Baltimore, through the courtesy of Mr. S. V. Grancsay.)

My comments about the manufacture of wire evidently need clarification. I did not say that drawn wire was never used, only that it was not always used, even in Europe in the heyday of mail. Wire was being drawn for use in wool cards and fish hooks well before the time of the earliest mail under consideration, and there is plenty of written evidence antedating the well-known pictures that Mr. Burgess refers to. The cross-sections of most of my European samples had slag distributed in a way that indicated that an approximately circular section had been maintained for some time during the process of reduction, i. e., that the wire had been drawn extensively. The oriental wires, however, all showed evidence of having been cut from sheet or strip, and three of the European ones (Nos. 2, 3, and 5) had been drawn only a little, if at all. These sections showed only a slight distortion of the parallel lamellar distribution of impurities which characterizes a strip, as can be seen in Figures 4 and 5, for example. The European wires may have been drawn through a finishing die, but they had been reduced in cross-sectional area by certainly far less than 50

per cent after the metal had been slit or sheared from a strip. Some of the oriental links showed no distortion whatever of the ends of the fibers, and the metal originally at the corners of the cut rectangular section must have been removed, not compressed inwards. Although it seems surprizing to us that all wire was not drawn, the evidence on these few links is incontrovertible. It should be recalled that even nails were made by slitting until well into the nineteenth century.

On the question of closed links, of course I do not contend that some rings somewhere may not have been punched from sheet, but all of the closed links I examined had *definitely* been welded. Welds are easily visible externally on most of the rings when examined with a lens—the one at the right on Fig. 2 is typical—and moreover, the circumferential streaks of slag visible on plane sections were obviously the bent-around longitudinal structure of the strip or wire, with up and down distortions due to irregularities of thickness and hammer blows during welding. Drifting is a most unpromising way of producing flat-section rings, and in any case would be recognizable by the slag particles running radially at two parts of the circumference. The only non-circumferential slag streaks in my rings were unmistakably due to welding. The joint was not a butt weld but a lap join with considerable overlap, amounting to 180° or more. The smith must have done a considerable amount of shaping after welding, although most of the resulting rings were far more irregular in section than any drawn or slit wire would have been.

Mr. Burgess's die-struck rings with a "T" overlap will almost certainly show lateral flow of metal into the head when they are examined metallographically. I suspect that they are the product of a trueing operation on a welded ring, not a ring-cutting operation which would leave a different kind of flash.

The ability to make superbly engraved dies for stamping things like coins, jewelry, type matrices, or identification marks existed during the period under discussion and for many centuries before. However, it is one thing to sink a design on the face of a single punch or die, and it is relatively easy to make a hollow punch that will cut out either positive or negative circles from soft sheet metal held against a suitable flat backing, but it is quite another matter to make a workable assembly with three matching parts which would be needed for stamping washers, even if it could be used without a press.

The question of making rivet holes by punching or pressing cannot be definitely answered. If it could be established that there was an identity of location of the hole in relation to "finger-print" impressions of die imperfections it would support Mr. Burgess's view: otherwise the simpler, commoner punch seems rather more probable. There is a nice picture in the Nuremberg *Hausbuch* showing pliers similar to those of Mr. Burgess being used in the final clinching operation on a mail suit. I note that Mr. Burgess no longer believes in the neat little

split die for drawing sectioned wires (which was the item mostly responsible for my suggesting the nineteenth-century date), and I still believe that he has little evidence beyond their plausibility for any of his ring-making tools.

Mr. Reid's paper will be awaited with much interest by everyone concerned with armour. The power to which he refers in the mail factory of 1400 A.D. could, of course, have been used for many operations, not only wire-drawing but also for the operation of a stamp mill, bellows for a forge, a tilt hammer, a grindstone or polishing wheel, and perhaps even for shears. I fail to see any significant technological detail in Dürer's pleasing painting.

My note was not meant to be the last word on the manufacture of mail; it was merely to point out the utility of a technique and to raise some interesting questions, not to answer them definitely. Many more samples of known origin should be examined. I would be delighted to make comparative metallographic studies of a reasonable number of significant rings and other wire objects if Mr. Burgess or others care to send them to me.

CYRIL STANLEY SMITH
Institute for the Study of Metals
University of Chicago

Notes

A Confrence on Science Manuscripts, concerned with the preservation of manuscript materials of interest to historians of science and technology, was held in Washington, D. C., on May 5-6, 1960.

The conference was attended by a distinguished group of scientists, engineers, archivists, librarians, and historians. An unusually well-planned program of papers on and discussion of the nature of the problem occupied the first day, while the second day was devoted to ways and means of carrying out a program that would effectively do the desired job. Speakers included Chauncey D. Leake, President of A.A.A.S.; Wayne C. Grover, Archivist of the United States; Luther Evans, Director of the Brookings Institution; Richard Shryock, Librarian of the American Philosophical Society; and several scholars currently working in the field.

It was unanimously agreed that personal manuscripts, drawings, and notebooks, as well as material in institutional files and archives, should be preserved and that the material should be made readily available to scholars through the preparation and maintenance of various finding aids. The encouraging progress being made in the compilation of the National Union Catalog of Manuscripts by the Library of Congress is one example of the possibility of providing more adequate information to the scholar.

It was further agreed that many bodies of papers could be saved only if a vigorous and continuing program of education (of the people whose papers ought to be preserved) was successful in getting across the need for preservation of such materials.

The Proceedings of the Conference will be published through a grant from the National Science Foundation, and recommendations for a working organization capable of handling the many phases of the problem will be prepared by the organizing committee of the conference for consideration by the conference group in December, 1960.

Nathan Reingold, of the Library of Congress, originated the plan for the Conference and, with the moral support of an organizing committee, carried out the planning and arrangements so vital to a successful conference.

EUGENE S. FERGUSON

(Mr. Ferguson, Curator of Mechanical and Civil Engineering of the Smithsonian Institution, was the official representative of the Society for the History of Technology to the Conference on Science Manuscripts.)

• • •

The University of Manchester Extra-Mural (Extension) Department is arranging an Anglo-American Summer School in Industrial Archaeology for the two weeks, July 15-29, 1961. The Preliminary Notice pointed out the purpose of the Summer School and the desirability of Manchester as a center for the study of Industrial Archaeology as follows:

"While industrial archaeology is concerned with the industrial remains of any date, the most urgent need at the moment is for work on the neglected period from the sixteenth to the nineteenth centuries. As the pioneering stage of industrialisation in the world took place in Britain during this period, it is clear that the study of the surviving remains are of international importance. It is an urgent matter that the surveying and recording of such monuments should be carried out on the widest possible scale at an early date. With this end in view, the Council for British Archaeology has recently formed a Research Committee on Industrial Archaeology to coordinate the work of the many different groups and individuals interested in the subject from such points of view as that of the technologist, the economic historian, the geographer, the architect and planner, and so on. A pilot survey of Staffordshire will soon be undertaken in collaboration with the County Council, and smaller surveys are being organised in Lancashire and Gloucestershire. The preparation of a handbook to guide the amateur industrial archaeologist is well under way.

"Manchester is an excellent center for the study of Industrial Archaeology. The city possesses the first important British canal, the Bridgewater, and the first railway station in the world, the terminus of the Liverpool and Manchester. In the immediate vicinity are excellent examples of early factory buildings as well as important specimens of the work of the canal and railway engineers. The early industrial areas of the West Midlands, with important sites such as Coalbrookdale and Wedgewood's original works at Etruria, the cotton mills of Derbyshire, and the docks and warehouses at Liverpool are in easy reach for day trips. The University has the oldest Chair of Economic History in Great Britain and is a leading center in research and teaching of the subject. A feature of the course will be organized excursions to sites of interest over a wide area of northern and central England."

Dr. E. R. R. Green of Manchester University History Department and Secretary of the Industrial Archaeology Research Committee of the Council for British Archaeology, will be Director of Studies of the Summer School. Further details will be available by early October from the Director of Studies, Extra-Mural Department, The University, Manchester 13, England.

* * *

The Xth International Congress of the History of Science will be held in the United States of America, 26 August-2 September, 1962.

Opening sessions of the Congress will be held at Cornell University, Ithaca, New York, and the concluding sessions will be held at the American Philosophical Society, Philadelphia, Pennsylvania. The President of the Congress is Professor Henry Guerlac of Cornell University. The Secretary of the Congress is Professor C. Doris Hellman.

All inquiries should be addressed to *The Secretary, Xth International Congress of the History of Science*, Cornell University, Ithaca, New York (U. S. A.). Those wishing to receive bulletins concerning the congress are requested to communicate with the Secretary.

* * *

Sociologie du Travail, a new French quarterly journal, began publication in the Fall of 1959. It contains articles of empirical research, scientific analysis, and discussion of the following topics: industrial sociology, the sociology of organizations, and the sociology of industrial relations. Published by the Association pour le Développement de la Sociologie du Travail, the journal is edited by Michel Crozier, Jean-Daniel Reynaud, Alain Touraine, and Jean-René Tréanton.

Among the articles in the first issue were: Georges Friedmann, "Quelques problèmes de définition et de limites,"; Yves Delamotte, "Conflit industriel et participation ouvrière"; Serge Moscovici, "La résistance à la mobilité géographique dans les expériences de reconversion"; and Jacques Dofny, "L'étude des changements techniques." The second issue contained articles by Michel Crozier, Nicole Leplatre, Alfred Willener, and others.

Editorial offices of *Sociologie du Travail* are located at 10, rue Monsieur-le-Prince, Paris VI^{e}; business offices are at Editions du Seuil, 27, rue Jacob, Paris VI^{e}. Annual subscription is 2400 francs.

* * *

Dr. David B. Steinman has appointed Dr. Paul Leser, Professor of Anthropology at the Hartford Seminary Foundation, as the official representative of the Society for the History of Technology to the Sixth International Congress of Anthropological and Ethnological Sciences in Paris, June 31-August 7, and to the International Congress of Historical Sciences in Stockholm, August 21-28, 1960.

* * *

Mr. S. Reid Warren, Assistant Vice-President for Engineering Affairs of the University of Pennsylvania, and Mr. John W. Streeter, of Streeter Enterprises in Philadelphia, were the official representatives of the Society for the History of Technology to the sixty-fourth annual meeting of the American Academy of Political and Social Science, held in Philadelphia, April 8-9, 1960. The theme of the meeting was "Whither American Foreign Policy?"

* * *

Mr. W. Earl Graham of Westfield, New Jersey, has taken over the Chairmanship of the Society's Museum Committee. The other members of this committee are Mr. Eugene S. Ferguson of the Smithsonian Institution and Mr. W. Stephen Thomas, Director of the Rochester Museum of Arts and Science. This committee is cooperating with the Committee for Museums of Science and Technology of the International Council of Museums, which has undertaken the task of issuing a list of museums of exact science and technology all over the world and of publishing a guide for the establishment of such museums where they do not yet exist.

* * *

Dr. Carl W. Condit of Northwestern University, chairman of the Program Committee, has issued a *tentative* schedule for the Society's Annual Meeting, to be held in New York at the end of December, 1960, in conjunction with the American Association for the Advancement of Science.

A joint session on "Nineteenth Century Science and Technology" with the History of Science Society will open the program on Tuesday afternoon, December 27.

One of the outstanding features of the meeting will be a joint program with the American Historical Association on Wednesday morning, December 28, on the topic "Technology as Cause in History." Lewis Mumford, author of *Technics and Civilization* and many other works, will speak on "Technological Determinism," and Roger Burlingame, author of numerous books on the development of technology, will discuss "Technology, the Forgotten Cause." Commentator on their papers will be Professor H. Stuart Hughes of Harvard University.

Wednesday afternoon, Professor Thomas P. Hughes, Washington and Lee University, will talk on "The British Electrical Industry in the Nineteenth Century: A Case of Retarded Growth," and Dr. Arnold R. Daum, Associate Director of the Petroleum Research Project at Northwestern University, will speak on "New Evaluations in the History of the Petroleum Industry in the Nineteenth Century." Dr. Bern Dibner of the Burndy Library will comment on these papers.

The annual business meeting of the Society will be on Thursday morning, December 29, to be followed by a luncheon where Dr. David B. Steinman will deliver his presidential address. "Automation" will provide the subject matter for the Thursday afternoon session, and Professor James R. Bright of Harvard University will be one of the principal speakers.

With the exception of the joint program with the American Historical Association, the program sessions will probably be held at the Belmont-Plaza Hotel, 49th and Lexington. The Housing Bureau of the AAAS will handle the housing arrangements for the members of

the Society; a housing coupon will be distributed well in advance of the meeting. Members may be housed at the same flat low rates at the Commodore, Biltmore, Roosevelt, and Belmont-Plaza. Special rates, although slightly higher, will also be available at the Waldorf-Astoria.

Chairman of the Local Arrangements Committee is Theodore B. Merrill, Jr., Industrial Production Editor of *Business Week*. He is assisted by L. Scott Bailey, Editor of *Antique Automobile* and Public Relations Counsel of the Eastern Railroad Presidents Conference, and by I. Jordan Kunik, patent attorney of New York City. Dr. Condit's colleagues on the Program Committee are Thomas P. Hughes and John W. Streeter.

* * *

It is not surprising that members of a learned society should be writing books and articles, but those by members of the Society for the History of Technology in the past few months are indeed noteworthy in quality, quantity, and diversity.

Two of our members have produced works of fiction. Theodore H. White, author of *Fire in the Ashes* and *The Mountain Road* (recently made into a movie), has produced another best-seller in *The View from the Fortieth Floor* (William Sloane Associates). L. Sprague de Camp, who has written some thirty-odd books of fiction and non-fiction, including several on technological subjects, has just published an historical novel about one of the Seven Wonders of the Ancient World—the Colossus of Rhodes. The novel, entitled *The Bronze God of Rhodes* (Doubleday), contains a great deal of information regarding the technology of the Hellenistic period.

Non-fiction titles produced by members of the Society include the following: James Kip Finch, *The Story of Engineering* (Doubleday-Anchor); Richard H. Shryock, *Medicine and Society in America, 1660-1860* (New York University Press); Carl W. Condit, *American Building Art: The Nineteenth Century* (Oxford University Press); Joseph Needham, Wang Ling, and Derek J. de Solla Price, *Heavenly Clockwork: The Great Astronomical Clocks of Medieval China* (Cambridge University Press); I. Bernard Cohen, *The Birth of a New Physics* (Doubleday-Anchor); Eugen Diesel, Gustav Goldbeck, and Friedrich Schildberger, *From Engines to Autos: Five Pioneers in Engine Development and Their Contributions to the Automotive Industry* (Henry Regnery Co.). Reviews of these books will be appearing in forthcoming issues of *Technology and Culture*.

The output of articles has also been impressive. A partial list would include the following: David Riesman (with Staughton Lynd), "The Relevance of Thorstein Veblen," *The New Statesman* (April); Peter F. Drucker, "Politics for a New Generation," *Harper's* (July); Allan Temko, "Russians in California," *American Heritage* (April); Eugene M. Emme, "The Impact of Technical Change and Western Military

Thought, 1914-1945," *Military Affairs* (Spring); William Christian, "Myth of the 'Electronic Brain,'" *Management and Business Automation* (June); James Kip Finch, "De Marchi to Digges . . . The Military-Civil Transition," *Consulting Engineer* (April) and "Agricola: De Re Metallica," *Consulting Engineer* (June); Sidney M. Edelstein, "An American Industry First—Rare Document Proves Papermaker First to Use Chlorine for Bleaching in the United States," *Tappi* (April); Joseph Rossman (with Barkev S. Sanders and L. James Harris), "The Growing Importance of Chemical in Comparison with Mechanical Patents," *The Patent, Trademark, and Copyright Journal of Research and Education* (Spring); Rudolf Clemen, "Beacons for the Weary Traveler," *American Heritage* (June); Willy Ley, "Horizons of Science and Engineering," *Senior Scholastic* (May); Joseph J. Ermenc, "The Convergence of Engineering and Liberal-Arts Education," *Journal of Engineering Education* (April); Melvin Kranzberg, "What Constitutes an Industrial Revolution?" *Foreign Policy Bulletin* (April).

More complete lists of the current writings of members will appear in subsequent issues of *Technology and Culture* and in the Society's Newsletters.

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